

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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No. 8.

Professor ADAMS, President, in the Chair.

Wm. E. Mitchell, Esq., The Fort, Norquay, Cornwall;
C. R. Tompkins, Esq., H.M. Dockyard, Portsmouth; and
G. H. With, Esq., Hereford;

were balloted for and duly elected Fellows of the Society.

The Society having Removed from Somerset House, all Letters, Parcels, and Communications are in future to be addressed
ROYAL ASTRONOMICAL SOCIETY, BURLINGTON HOUSE,
Piccadilly, W.

Note on a Paper by Mr. Christie, "On a Method of connecting the Curvature of the Lines in the Dispersion Spectrum."

By William Simms, Esq.

In an investigation into the cause of the curvature of the lines in the Dispersion Spectrum, March No., p. 263, Mr. Christie has shown that a correction may be obtained by a direct reflexion through the same plane; but when the ordinary right-angled prism is used at the end of the train for the purpose of returning the spectrum, the evil is propagated, precisely as it would be had it followed its original course through a similar number of prisms.

Upon reading Mr. Christie's paper, the idea occurred to me, that by employing a prism of three reflexions, the correction would also be obtained, carrying on the spectrum at a higher plane, as with the right-angled prism; and so this improvement could be readily made to existing spectroscopes.

Mr. Christie has suggested, that to avoid the loss of light from the middle portion of the train, the upper and lower halves should be divided, raising the upper halves; this might be desirable in a new instrument, but would be a costly alteration to an old one. I think there would generally be sufficient light without having recourse to this expedient; if thought desirable, a detachable prism might be applied, and this, or the two reflexion prisms, used at pleasure.

Burnham,
1874, May 4.

Professor C. V. Zenger, in two letters addressed to Mr. Glaisher, dated Prague, January 17 and March 22, 1874, proposes a method of enlarging lunar and solar photographs so as to correct the spherical aberration, and makes suggestions as to the use of photographs in the coming transit of *Venus*.

In the first instance he gets very sharp images by using a parabolic spectrum bought from Mr. Browning, the rest of the spherical aberration being destroyed by a very fine aplanatic lens from Steinheil with negative focal length. He got in this way solar images with spots on them of, perhaps, the greatest sharpness ever obtained, with faculæ, willow leaves, and a most sharply defined gradation of penumbra, the diameter of the Sun's image being from 20 to 24 inches.

He then conceived that the remainder of aberration got in the images might be easily destroyed by photographing with lenses whose remainder of error was opposite to that of the photographing speculum or refractor, and tried the experiment with much delight, on a fine sample of Rutherford's 15-inch lunar photograph, with a very good, but a little over-corrected, opera-glass from Paris, magnifying 8 to 10 diameters. Not only the lunar photograph got a wholly improved aspect, the influences of unevenness being nearly destroyed, (falling in the opposite direction to the spherical aberration), but objects could be detected in it invisible, or very uncertain, even with the aid of an excellent magnifier by Steinheil. Ridges, shadows, and inclined planes, slopes of the *illuminated* and uneven surface of the inner parts of the craters, not or scarcely visible with the naked eye and magnifying aplanatic lens, became as sharp and definite as he could see them in one of the clearest and driest winter nights with Browning's 5-inch reflector with 100 and 300 magnifying power.

Some lunar photographs originally 3 inches in diameter, and enlarged by Brothers of Manchester to 11 inches, Professor Zenger has in this way enlarged to 110 inches, without losing, and indeed gaining, definition. Specimens (on paper) of *Clavius* and its vicinity accompanied the letters, and were exhibited at the meet-

ing of the Society; but he proposes to bring with him to the ensuing meeting of the British Association glass positives on the scale of 80 to 110 inches.

He concludes with suggestions as to photographing the transit:

1°. It is of no use to photograph the moment of the contact, because accuracy is entirely destroyed by the effect of the interference.

2°. It would be better to have the passage photographed at intervals so as to ascertain exactly the moment at which *Venus* passes through a determined meridian on the Sun's surface. The Sun's apparent diameter being nearly the Moon's or 31', it can by his process easily be enlarged to 110 or more inches diameter, one inch corresponding to $\frac{31'}{110} = 0'.0282$ or 1".7. By subdividing to one-hundredth, it would be possible to ascertain the position of *Venus* on the Sun's disc up to 0".017 or 0".001 in time, an amount of precision scarcely obtainable by the method proposed by M. Janssen at the last meeting at Bradford. But he conceives the magnifying might even go to 200 or 250 inches.

On a remarkable Structure visible upon the Photographs of the Solar Eclipse of December 12, 1871.

By A. Cowper Ranyard, Esq.

The structure which I am about to describe is by no means a marked feature on the Indian photographs; indeed, it was not observed until after nearly a year had been spent in cataloguing the details which are to be made out on the different negatives. When, however, it has once been pointed out, no careful observer can have any doubt as to its existence; and the tardiness with which it was observed may perhaps be accounted for by the fact that attention was principally directed to an examination of the dark or partially opaque details of the photographs which correspond to the luminous details of the corona, whereas this was a bright or transparent structure; and bright spots, lines, or patches had always been regarded as photographic defects, and consequently but little attention had been paid to them.

The original negatives are very small: the dark moon is represented by a transparent circle about $\frac{3}{10}$ ths of an inch in diameter, and the whole extension of the corona could be covered by a sixpence. The separate details of the coronal structure are therefore very minute, and it would be impossible from the examination of a single negative to determine whether any small marking has its origin in some almost microscopic impurity on the collodion, or whether it represents a vast mass of many million cubic miles in

the corona: it is only by a careful comparison of the different negatives that such photographic flaws can be properly eliminated. For this purpose a Catalogue has been made, containing a list of the negatives upon which each detail can be distinguished, and the details entered in the catalogue have also been drawn. In this work I have been fortunate enough to be aided by a most accurate and conscientious artist (Mr. Wesley), for whose laborious perseverance in the task I cannot be too thankful.

All the details cannot be seen on any one negative; in some the structure of the lower parts of the corona is all that can be made out, while in others the middle heights are best seen, the extreme extension of the corona being lost, and the lower parts merged in an opaque mass by reason of over-exposure. Again, on any one negative, all the details cannot be seen at the same time, they only become visible as the plate is examined with different amounts of transmitted light, and with different magnifying powers; for example, the prominence structure which extends to a height of 2' or 3' can only be made out with strong transmitted light, and a moderately high magnifying power, whereas the structure of the corona at a height of from 10' to 15' can only be seen on the background of a bright sky, and is completely lost when a lens is used. The very furthest extensions of the corona which can in some instances be traced to a distance of quite 26' from the Sun's limb can only be seen by reflected light. In making the catalogue no details have been entered or drawn which could not be traced on three of the negatives.

While working at a group of coronal structure* on the eastern equatoreal limb, Mr. Wesley noticed that a small bright spot, or flaw as we then considered it, occupied apparently the same position in negatives 1 and 4 of Lord Lindsay's series. On examining the others of Lord Lindsay's or the Baikul series we found that a bright spot or flaw was more or less distinctly to be traced in the same place on all of them.

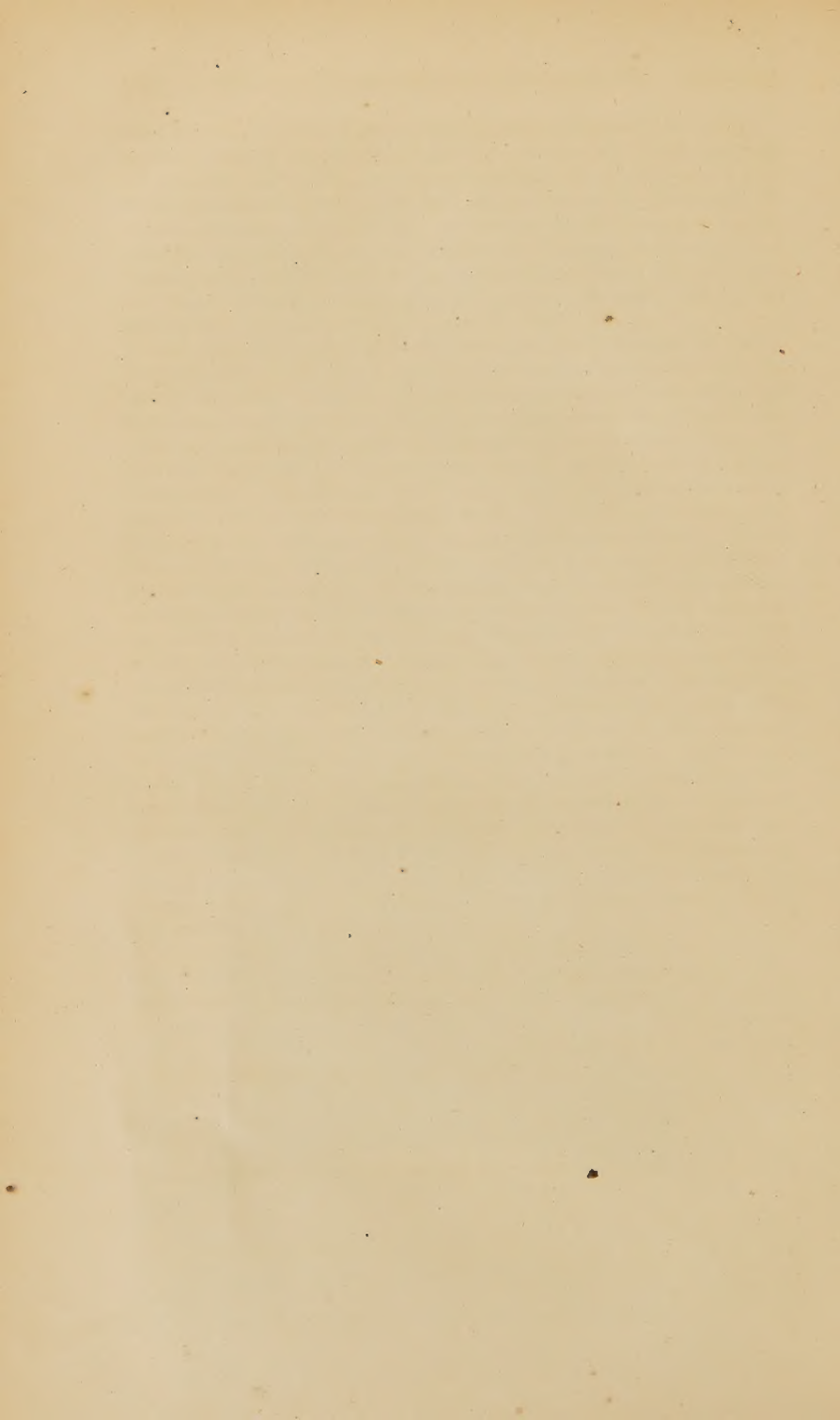
I at first thought that it must be due to a star seen through the corona, but a little reflection showed us that this explanation could not be sustained, for the image of a star would have been represented by a dark or very opaque point, whereas this was bright: and therefore, the collodion had at this point been less acted upon than by the light from the surrounding details. On a closer examination of the plates upon the next fine day, three partially transparent circular arcs, concentric with the bright spot, were detected above it. The middle one of the three arcs is the most distinct, and can be traced without any doubt upon four out of the five Baikul negatives.

* It should be noted that in the plate appended to this paper the distinctness of the coronal details is intentionally exaggerated. Not only would it have been very difficult to reproduce by lithography the delicate differences of intensity which are traceable in the negatives, but, if it had been possible, the plate would not have so well served the purposes of a diagram.



STRUCTURE VISIBLE UPON THE PHOTOGRAPHS
OF THE ECLIPSE OF DEC. 12TH 1871.

(Black representing opacity in the Negatives)



This circular and concentric structure is so different from all the other forms traceable among the dark details of the corona, that I was loath to accept it as being in any way connected with the Sun. The idea struck me that it might be due to reflexions taking place within the camera, but such reflexions would always have been symmetrically situated with regard to the camera, and therefore always have fallen on identically the same part of the plate; whereas, although the structure is always to be found on identically the same part of the corona, the image of the corona is shifted upon the different plates. Again, any reflexions of a bright image or ghost would have given rise to a dark structure upon the photographs. There seemed, therefore, no alternative but to suppose that the structure was due to some partially opaque body situated between us and the Sun, cutting out or partially intercepting the light of the corona. At this time I had only Lord Lindsay's negatives and two enlarged copies of the Java photographs, to which I could refer. As all traces of the structure were lost on the copies of Lord Lindsay's negatives, I was not surprised to find that it could not be seen on the Java enlargements. After a short time I obtained the loan of Colonel Tennant's original negatives from Mr. De La Rue. They were taken at Ootacamund, a distance of more than 120 miles from Baikul, and I was therefore not a little astonished and pleased to find that the central bright spot was traceable on five out of the six negatives of his series. The central arc was also just traceable on four out of the six negatives, and the inner arc is to be made out on three of the negatives.

In the catalogue the central bright spot has been lettered E. ϵ . The most distinct of the three arcs is lettered E. ζ ., and the other two arcs are taken together under the heading E. η . The descriptions run thus :—

E. ϵ . (Position-angle 85° .) A minute bright spot about $9'$ from the Sun's limb. It appears to encroach on the Northern edge of E. β , and to touch the top of E. δ . It is best seen upon the background of a clear sky.

B.1. (*i.e.* negative No. 1. of the Baikul series) distinctly visible.

B.2. Distinctly visible.

B.3. Distinctly visible, though not so clearly marked as in B.1.
B.2. and B.4.

B.4. Distinctly visible.

B.5. Just visible.

O.1. (*i.e.* negative No. 1. of the Ootacamund series) visible.

O.2. Distinctly visible.

O.3. Distinctly visible.

O.4. Only just to be made out and not with certainty.

O.5. Only just visible.

O.6. Lost.

E.ζ. A bright arc about $1\frac{1}{2}'$ broad, with a radius of about $6'$, it appears to be concentric with E.ε., and is concave towards the Sun. At its highest part it is about $16'$ above the Sun's limb. On the South it cuts across E.γ. E.β. E.α. and on the North extends to a distance of about $4'$ on to the area of D.η.

B.1. Very distinctly visible.

B.2. Quite lost.

B.3. Just visible.

B.4. Very distinctly visible.

B.5. Distinctly seen, though not so clear as in B.1. and B.4.

O.1. Visible, though its central portions are interfered with by a streak (the streak is evidently due to drainage action which took place while the plate was wet).

O.2. Just visible, at first sight it appears to be traceable down as far as the Southern branch of D.ε., but possibly this part of the curve is formed by a photographic defect. It gives a distinctly parabolic appearance to the arc.

O.3. Distinctly visible.

O.4. Distinctly visible.

O.5. Lost.

O.6. Lost.

E.η. The traces of a circular arc much fainter than E.ζ. It appears to be concentric with E.ε., and has a radius of about $3'$. The rays E.α., E.β., and E.γ. can be distinctly traced through it, and appear almost to break it up into three bright spots. An outer concentric circular arc with a radius of about $10'$ is also just visible, but as it can only just be traced upon three of the negatives, a separate letter has not been given to it in the catalogue.

B.1. Distinctly visible; the outer arc is also to be seen.

B.2. Quite lost.

B.3. Just traceable.

B.4. Distinctly visible; the outer arc is also to be traced.

B.5. Just visible with a good light.

O.1. Distinctly visible with a good light, though it is cut across by an opaque streak.

O.2. Just visible.

O.3. Distinctly visible; the outer arc is also to be traced.

O.4. Lost.

O.5. Lost.

No difference can be detected in the position of the central spot and concentric arcs relatively to the details of the corona in passing from the Baikul to the Ootacamund series; and taking into consideration the distance between the two stations, it is evident that the structure must be either due to some dark body in the corona, or to some semi-transparent body situated between us and the corona, at a great distance from the Earth.

The form of structure is similar to that which has often been

observed in the nuclei and the concentric comæ of comets; and it seems not very unreasonable to suppose that this may really be a photograph of a faint though large comet near to perihelion.

On this supposition, it is not perhaps very remarkable that the structure should not have been detected by the many observers who were engaged in examining the corona during totality—for if the comet was similar in chemical composition to those whose spectra have been observed by Mr. Huggins and Professor Young, the loss of light caused by its absorption would have been more readily detected on the photographs than by the human eye, for the two most refrangible bands of the carbon comets lie in parts of the spectrum which produce but a feeble effect upon the retina, though the actinic action is there very strong. The two series of photographs are not sufficiently removed from one another in point of time to show any motion of the comet over the details of the corona. The date of Colonel Tennant's No. 1 negative is about one minute of time after Lord Lindsay's No. 1; and Colonel Tennant's No. 5 is about three minutes after Lord Lindsay's No. 1; but we have been unable to trace any difference in the position of the nucleus in passing from Lord Lindsay's No. 1 to Colonel Tennant's No. 5.

It is worthy of remark that a somewhat similar structure was observed by Professor Winnecke during the Eclipse of 1860. A drawing of it has been published in the *Mémoires de l'Académie Impériale de St. Pétersbourg*, VII^e Série, Tome IV., No. 1. It consisted of a single parabolic arc, with its convexity turned towards the Sun, and appeared as if drawn with sepia upon the bright background of the corona. Dr. Winnecke describes it, at page 39 of the above Memoir, as "einen parabolisch gekrümmten, dunklen Bogen im Lichte der Corona. . . . Dieser Bogen schien mir gleichsam mit Sepia auf dem lichten Grund der Corona gezeichnet zu sein."

An undoubted comet has been seen projected on the brilliant background of the photosphere (I refer to Pastorff's observations of the Comet of 1819*). And much fainter comets would be visible when projected on the comparatively feeble light of the corona. It seems, therefore, not very improbable that both this structure and the parabolic arc observed by Dr. Winnecke may have been due to comets which happened to be situated between us and the corona during times of total eclipse. And it is not impossible that such giant comets may exist in great numbers in the immediate neighbourhood of the Sun, though by reason of their faintness, or the short time of their ebullition, they are not visible to us either before or after their perihelion passage.

* As well as to the nebulous spot observed by him upon the Sun in May 1828. See the *Monthly Notices*, November 1873.

Notes in reply to Sir John Herschel's "Queries relative to Double Stars." By S. W. Burnham, Esq.

The following Queries as left by the late Sir John Herschel were printed in the *Monthly Notices* for November 1871, by the Committee appointed by the Royal Astronomical Society, to examine and report upon a collection of papers bequeathed by him to the Society for the preparation of a "General History of Double Stars," with the request that observers should aid in removing the doubts existing in reference to these stars. As stated in the recent Report of the Council, one of the forthcoming volumes of *Memoirs* will contain this very valuable and needed contribution. I was not aware until within a few months of these Notes of Herschel; but some time previously prepared a list of several hundred double stars requiring telescopic examination, and which included most of the following objects. The list might be very much extended. I select from a large mass of notes such as pertain to the double stars in question. The observations have been made exclusively with a 6-in. Clark refractor.

$$\hbar 2021 \left\{ \begin{array}{l} \text{R.A.} = \overset{\text{h}}{1} \overset{\text{m}}{0} \overset{\text{s}}{37} \\ \quad \quad \quad \circ \quad \quad \quad \text{''} \\ \text{P.D.} = 109 \quad 31 \quad 40 \end{array} \right\} \text{Is it really close double?—"Violent suspicion.}$$

Examined on only occasion only, but no double star found in this place. The night, however, was not first class, and if a *very* difficult object, it might not have been seen.

$$\approx 168 \left\{ \begin{array}{l} \text{R.A.} = \overset{\text{h}}{1} \overset{\text{m}}{40} \overset{\text{s}}{17} \\ \quad \quad \quad \circ \quad \quad \quad \text{''} \\ \text{P.D.} = 20 \quad 47 \quad 30 \end{array} \right\} \text{Not seen double by } \hbar.$$

I have just noticed for the first time, that the above P.D. as given by Herschel is incorrect. It should be, $24^{\circ} 7' 31''$. I have looked for this pair several times, but I think on each occasion used Herschel's P.D., and of course failed to find it. Herschel's observation occurs in his Fourth Catalogue, and there the place is correctly stated, so that it was properly searched for by him. It is now too low to do anything with.

$$\approx 211 \left\{ \begin{array}{l} \text{R.A.} = \overset{\text{h}}{1} \overset{\text{m}}{55} \overset{\text{s}}{54} \\ \quad \quad \quad \circ \quad \quad \quad \text{''} \\ \text{P.D.} = 96 \quad 14 \quad 50 \end{array} \right\} \text{No such star found in or near place by } \hbar.$$

I could not find any star as bright as 8th mag. in this place: there are some small stars, but none of them were seen double. This is one of Struve's rejected pairs, and is not included in *Positiones Mediæ*, or *Mensuræ Micrometricæ*. It is given in Struve's early Catalogue as Class IV., and magnitudes (8), (10).

Σ 343	{	R.A. = 2 ^h 55 ^m 23 ^s	}	Are these the same double star? Σ is confused, and h disagrees.
		P.D. = 6 35 32		
Σ 347	{	R.A. = 2 ^h 56 ^m 27 ^s	}	
		P.D. = 6 35 2		

In *Mensuræ Micrometricæ* it is stated that the two are identical, there being an error of 1^m in the R.A. of the second. Dembowski's measures of this pair show a slight change in the distance of the components, the angle remaining the same. Struve found, P = 325°.4; D = 22".66, (1832.6). Dembowski gives, P = 325°.2; D = 24".95 (1865.0). I am not acquainted with any other measures.

Σ 407	{	R.A. = 3 ^h 21 ^m 56 ^s	}	Is the minute of R.A. 19 or 21? Σ's two catalogues differ.
		P.D. = 101 43 4		

The place of this pair appears to be the same (3^h 21^m.8) in both Struve's Catalogues. It is not in *Positiones Medice*. I found it, readily identified it as L 6490, the place of which for 1830 is:

$$\text{R.A.} = 3^{\text{h}} 21^{\text{m}} 52^{\text{s}}$$

$$\text{N.P.D.} = 101^{\circ} 43' 40''$$

Σ 417 Σ 518	{	R.A. = { 4 ^h 7 ^m 27 ^s 4 ^h 7 ^m 48 ^s	}	40 Eridani. Confusion exists. Wanted an exact diagram, description, and measures of A, B, C; and ? of a fourth star D.
		P.D. = { 97 55 22 97 54 23		

Struve's place for 1830 (*Pos. Med.*) is:

$$\text{R.A.} = 4^{\text{h}} 7^{\text{m}} 27^{\text{s}}$$

$$\text{P.D.} = 97^{\circ} 55' 22''$$

This agrees exactly with the British Association Catalogue. There are four companions, the measures of which are as follow:—

	Pos. °	Dist. "	Mags.	
A and B	107.2	83.48	4, 9.1	Struve (1836.0)
B „ C	159.9	3.±	10½, 11	Dawes (1851)
A „ D	185.0	75.85	...	Winnecke (1864.8)
A „ E	312.5	89.45	...	Winnecke (1864.8)

Engelmann for A and B (1863.1) gives 105°.4; 82".83. The close pair, constituting a binary system in rapid orbital motion, was found by Winnecke (1864.8) to give P = 147°.6; D = 4".45.

Knott (1871) estimates the angle and distance $125^{\circ} \pm : 2'' \pm$. My 6-inch refractor shows readily all these stars, except perhaps E, which was not particularly looked for. Struve speaks of a 12 mag. star at $16''\cdot79$ in the direction of $96^{\circ}8$. I do not find any later or other mention of this star.

$$\approx 609 \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{ccc} h & m & s \\ 4 & 41 & 59 \\ & ^{\circ} & ' & '' \\ \text{P.D.} = 89 & 8 & 59 \end{array} \end{array} \right\} \begin{array}{l} \text{? whether Struve's place, as here given from } \textit{Pos.} \\ \textit{Med.}, \text{ or that of my } \textit{Seventh Catalogue}, 4^h 43^m 4^s, \\ 89^{\circ} 3' 17'', \text{ be right. Positions and distances} \\ \text{agree. Or are they really two different double} \\ \text{stars? My place is corroborated in } \textit{Fifth Cata-} \\ \textit{logue}, h 2240 \text{ bis, making the R.A. } 43^m. \end{array}$$

I examined this for the purpose of seeing if it was identical with *h* 686, given by Herschel without measures or magnitudes, with this note: "Close double; taken for Σ 609, but clouds prevented its description." I looked for it on a very superior night, but could not, or did not see it. Subsequently I found it very readily, and noted it as "very easy," estimating independently the distance at $2''$, and the angle 80° . Struve gives $D = 1''\cdot93$; $P = 82^{\circ}\cdot1$. There is something rather strange about this pair, unless by some oversight Dembowski missed it. He has this remark in his measures of double stars made in 1865'9: "Simple; bonnes conditions." He also found it single, 1866'1. Struve's mags. are 8'5, 8'7, which should make it, as I found it, an easy pair. Without ascertaining positively, it appears to be L 9101, the place of which agrees exactly with *Pos. Med.* I could not find any other pair in the neighbourhood, and have no doubt *h* 686 is the same as Struve's pair. Herschel's declination of that star agrees with Struve.

$$\approx 662 \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{ccc} h & m & s \\ 5 & 5 & 39 \\ & ^{\circ} & ' & '' \\ \text{P.D.} = \begin{array}{ccc} 94 & 15 & 54 \\ 64 & 15 & 54 \end{array} \end{array} \right\} \begin{array}{l} \text{In } \textit{Pos. Med.} \text{ the P.D. is } 94^{\circ} 15' 54'', \text{ but in the} \\ \textit{Dorpat Catalogue}, \text{ and also in the } \textit{Mens. Mic.}, \\ \text{the degree of P.D. is } 64^{\circ}. \text{ The declinations} \\ \text{given are: } \textit{Dorpat}, +25^{\circ} 45'; \textit{Mens. Mic.}, \\ +25^{\circ} 45'; \textit{Pos. Med.}, -4^{\circ} 15' 54''. \end{array}$$

The place in *Mens. Mic.* is correct. The companion, although rated by Struve as 11th magnitude of his scale, was well seen with a power of 400, and is one of those minute stars which are improved by magnifying. There is no double in the place set down in *Pos. Med.* The star in question is L 9809.

$$h 5465 \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{ccc} h & m & s \\ 5 & 39 & 18 \\ & ^{\circ} & ' & '' \\ \text{P.D.} = 78 & 4 & 3 \end{array} \end{array} \right\} \begin{array}{l} \text{An excessively minute comes } 12'' \text{ distant suspected.} \\ \text{? if verified.} \end{array}$$

I have looked for this several times, once when the conditions were remarkably favourable, but have never been able to see the least trace of a companion. If it exists at all, which is very doubtful, it must be an object of excessive difficulty.

$$\Sigma 923 \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{ccc} \text{h} & \text{m} & \text{s} \\ 6 & 22 & 18 \\ 6 & 42 & 15 \\ 0 & ' & '' \end{array} \\ \text{P.D.} = 30 \quad 25 \quad 8 \end{array} \right\} \text{? which is the right minute of R.A. } \Sigma \text{ leaves it doubtful, and I have no observation of it.}$$

This is one of Struve's rejected pairs, described in *Catalogus Novus* as Class IV. (6), (10). The correct minute of R.A. is 21. I am not aware of any measures having ever been made of this pair. The companion is too distant to make it a very interesting object, and for this reason it was not included in *Mens. Mic.* The companion is in the direction of about 140° . There is a more distant companion *s* a little *p*.

$$\Sigma 1006 \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{ccc} \text{h} & \text{m} & \text{s} \\ 6 & 51 & 22 \\ 0 & ' & '' \end{array} \\ \text{P.D.} = \begin{array}{ccc} 26 & 59 & 18 \\ 27 & 10 & 18 \end{array} \end{array} \right\} \text{Which is the right P.D.? In the } \textit{Dorpat Catalogue} \text{ the declination is } +63^\circ 1'; \text{ in the } \textit{Mens. Mic.}, +62^\circ 50'. \text{ Which is right?}$$

This is a very wide pair ($30'' \cdot 59$) of moderately bright stars, both of which are given in Argelander's *Zones = Arg.* ($+62^\circ$) 902, 904. The place for 1855 of the principal star is:

$$\text{R.A.} = \begin{array}{ccc} \text{h} & \text{m} & \text{s} \\ 6 & 53 & 27 \end{array}$$

$$\text{P.D.} = \begin{array}{ccc} 0 & ' & '' \\ 27 & 14 & 6 \end{array}$$

This does not agree exactly with either of Struve's Catalogues. I have not seen any measures of this pair made since Struve's observations.

$$\Sigma (989) \text{ } g. c. \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{ccc} \text{h} & \text{m} & \text{s} \\ 8 & 14 & 22 \\ 8 & 14 & 25 \\ 0 & ' & '' \end{array} \\ \text{P.D.} = \begin{array}{ccc} 92 & 25 & 7 \\ 92 & 23 & 51 \end{array} \end{array} \right\} \text{Set down as "very near" } \sigma 295 \text{ (} \textit{Catalogi Prioris} \text{). Are these really two different double stars? If not, which is right?}$$

The first refers to the general number in *Positiones Medice*. There is but one pair in this place, and that is a very wide double, the companion nearly preceding, and about the 10th mag. The star is L 16441, and the place for 1830 from that Catalogue is:

$$\text{R.A.} = \begin{array}{ccc} \text{h} & \text{m} & \text{s} \\ 8 & 14 & 21 \end{array}$$

$$\text{P.D.} = \begin{array}{ccc} 0 & ' & '' \\ 92 & 25 & 0 \end{array}$$

agreeing with *Positiones Medice*. It is an uninteresting object, and so far as I am aware, has never been measured or included in any modern catalogue. I have carefully examined all the stars in the vicinity, and fail to find any other pair nearer than $\Sigma 1233$.

$$\Sigma 1259 \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{ccc} \text{h} & \text{m} & \text{s} \\ 8 & 32 & 7 \\ 0 & ' & '' \end{array} \\ \text{P.D.} = 51 \quad 5 \quad 29 \end{array} \right\} \text{In } \textit{Dorpat Catalogue} \text{ } 8^{\text{h}} 31^{\text{m}} \cdot 9, \text{ decl. } +38^\circ 56' \text{ in } \textit{Mens. Mic.} \text{ } 8^{\text{h}} 36^{\text{m}}, \text{ decl. } +39^\circ 5'. \text{ Query, Which is right? } h \text{ has no observation.}$$

The place given is from *Positiones Medice*, which differs from Struve's other Catalogues both in R.A. and P.D.; and strangely enough they are all in error, but that from *Mensuræ Micrometricæ* is the nearest and substantially correct. $\Sigma 1259$ is identical with Weisse VIII. 937, and its place for 1830 from that Catalogue is:

$$\text{R.A.} = \begin{matrix} \text{h} & \text{m} & \text{s} \\ 8 & 35 & 35 \end{matrix}$$

$$\text{Decl.} = +39^{\circ} 6' 3''$$

I did not find any star corresponding to the place in *Positiones Medice*—at least no star as bright as the 9th magnitude, while that star is rated by Struve as 7.5. Either the star is variable, or there is some error in the observation, probably the latter. Upon the first examination of this vicinity, I found a very pretty pair, which I then took to be $\Sigma 1259$, with a very considerable change both in angle and distance. For his pair Struve gives, $P=4''.97$; $D=340^{\circ}.9$. The distance of the pair found was certainly not greater than $2''$, and the angle obviously more than 340° . A mean of several measures, differing but slightly from each other, of the angle gave $355^{\circ}.5$. This star is Weisse VIII. 849, and the place for 1830:

$$\text{R.A.} = \begin{matrix} \text{h} & \text{m} & \text{s} \\ 8 & 35 & 5 \end{matrix}$$

$$\text{Decl.} = +39^{\circ} 24' 35''$$

Finding that it differed from *all* the places given of $\Sigma 1259$, I made a further search, and at once found Struve's pair in the place given above. On the first occasion I examined all the stars that were prominent in the finder, but this escaped attention from its faintness. It is hardly possible that Struve should have overlooked the new pair, unless some change has occurred since his observations. It is nearly one magnitude brighter than $\Sigma 1259$; and as the components are about equal, it makes a very easy object, and one not likely to be overlooked with any aperture above 3 inches.

$$\delta \text{ Cancri } \left\{ \begin{matrix} \text{R.A.} = \begin{matrix} \text{h} & \text{m} & \text{s} \\ 8 & 35 & 1 \\ & 0 & ' & '' \\ \text{P.D.} = 71 & 13 & 39 \end{matrix} \end{matrix} \right\} \begin{matrix} \text{h} 457 \text{ makes position } 160^{\circ}, \text{ distance } 25''. \\ \text{Lassell, } \textit{Astr. Nach.}, \text{ No. 858, states it as,} \\ \text{position } 121^{\circ} 12', \text{ distance } 45''. \text{ What is it} \\ \text{really?} \end{matrix}$$

The companion though rated by Herschel as 15 magnitude, was noted by me as "well seen with 400; the distance certainly more than $25''$ —probably not less than $45''$ ". The angle appears to be nearer 120° , but too minute to estimate with great certainty." Subsequently I made three measures of the angle, the mean of which gave $P=120^{\circ}$. The companion is very faint, but the separate results are sufficiently accordant ($120^{\circ}.5$, $118^{\circ}.5$, $121^{\circ}.5$) to make it apparent that Herschel erred in his estimate of the angle. I made no attempt to measure the distance; but it is obviously greater than the value assigned by Herschel, and undoubtedly Lassell's result is also correct in that respect.

$h\ 2616 \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{c} h \\ 12 \end{array} \begin{array}{c} m \\ 29 \end{array} \begin{array}{c} s \\ 0 \end{array} \\ \text{P.D.} = 75 \quad 17 \end{array} \right\}$ Close to a star of 9th mag. is a small, well-defined body, which may be a close double star. *np* is a very faint nebula (no doubt III. 602, or Neb. No. 3113 of my *General Catalogue*). Is it there still? Was it a planet?

In Herschel's "Queries" the name or number of the star is erroneously printed $\delta\ \textit{Canceri}$. It should be as I have given it, $h\ 2616$. This is a faint double star, and readily seen with a 6-in. aperture when the conditions are favorable. It is strange that Herschel should have had any doubt about it, unless his instrument was not properly in focus at the time, or the definition was very poor, when a close, faint pair might present some such appearance as that described. I estimated the angle of this pair as about 310° , and distance perhaps a little more than $2''$. Their 9th magnitude star near it is $L\ 23620$, given in *Lalande* as 8th magnitude (and by another observation 7) the place of which (1830) is:

$\text{R.A.} = \begin{array}{c} h \\ 12 \end{array} \begin{array}{c} m \\ 30 \end{array} \begin{array}{c} s \\ 2 \end{array} \quad \text{P.D.} = \begin{array}{c} ^\circ \\ 75 \end{array} \begin{array}{c} ' \\ 15 \end{array} \begin{array}{c} '' \\ 15 \end{array}.$

I measured the distance and angle of the faint pair from this star, obtaining $P=237^\circ$; $D=93''$. The very faint nebula referred to, I did not notice.

$\kappa\ \textit{Virginis} \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{c} h \\ 14 \end{array} \begin{array}{c} m \\ 3 \end{array} \begin{array}{c} s \\ 50 \end{array} \\ \text{P.D.} = 99 \quad 28 \quad 42 \end{array} \right\} \sigma\ 453\ (\textit{Catalogi Prioris}).$ Is it really a double star?

It is not properly speaking a double star. There are two distant companions, one nearly following, the other preceding, both too far away to make it an object of any interest. No measures have ever been made, nor is it found in any catalogue where measures are given.

$\omega\ \textit{Boötis} \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{c} h \\ 14 \end{array} \begin{array}{c} m \\ 54 \end{array} \begin{array}{c} s \\ 40 \end{array} \\ \text{P.D.} = 64 \quad 18 \quad 58 \end{array} \right\} \sigma\ 469.$ Is it really double?

There are some minute stars in the field, but they are too distant to be regarded as companions. Examined on a superb night; the star perfectly round, with 400. This is not included in any regular or original catalogue of double stars.

$h\ 1271 \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{c} h \\ 15 \end{array} \begin{array}{c} m \\ 15 \end{array} \begin{array}{c} s \\ 18 \end{array} \\ \text{P.D.} = 107 \quad 59 \quad 48 \end{array} \right\}$ Are these identical? and, if so, which P.D. (differing 1°) is the right? The measures agree.

$h\ 4768 \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{c} h \\ 15 \end{array} \begin{array}{c} m \\ 15 \end{array} \begin{array}{c} s \\ 18 \end{array} \\ \text{P.D.} = 109 \quad 0 \quad 43 \end{array} \right\}$

In the P.D. of the latter (from *Cape Observations*) there is undoubtedly an error of 1° . The measures and magnitudes of the two pairs correspond very closely. I found $h\ 1271$ at once in the place assigned, but could find no double star at all in the neighbourhood of 1° further south.

$$h\ 256 \left\{ \begin{array}{lcl} \text{R.A.} = 15^{\text{h}} 32^{\text{m}} 2^{\text{s}} \\ \text{P.D.} = 71^{\circ} 40' 0'' \end{array} \right\} \begin{array}{l} \text{Not found subsequently; but ? if properly looked} \\ \text{for. Does it exist? Measures } 5^{\circ} 3f; \text{ distance } 2''. \end{array}$$

I have not been able to find any such pair in this place. I have also examined the catalogues to see if there is any which might have been observed and the degree of P.D., or hour of R.A. erroneously noted, but do not find any pair likely to be the one in question. Herschel has not given the magnitudes of this pair.

$$\Sigma\ 2012 \left\{ \begin{array}{lcl} \text{R.A.} = 16^{\text{h}} 0^{\text{m}} 49^{\text{s}} \\ \text{P.D.} = 97^{\circ} 48' 13'' \end{array} \right\} \text{Is this really identical with B.A.C. } 5379?$$

This is one of Struve's rejected stars, and of course given in the early catalogue without measures. This is not B.A.C. 5379, but is L 29435, which is $48^{\circ} p$ and $2' 52'' n$. My estimates of angle and distance were: $P=270^{\circ}$; $D=15''$; mags. 8, 13. I have since found an observation of this pair in *Cape Observations*, where $P=256^{\circ} 7$; $D=20'' \pm$; mags. $8\frac{1}{2}$, 11. I know of no other measures. Herschel gives the place in that work for 1830:

$$\text{R.A.} = 16^{\text{h}} 0^{\text{m}} 49^{\text{s}}$$

$$\text{P.D.} = 97^{\circ} 47' 27''$$

The place (substantially the same) from Lalande for the same epoch is:

$$\text{R.A.} = 16^{\text{h}} 0^{\text{m}} 50^{\text{s}}$$

$$\text{P.D.} = 97^{\circ} 47' 54''$$

$$h\ 584 \left\{ \begin{array}{lcl} \text{R.A.} = 16^{\text{h}} \left\{ \begin{array}{l} 7 \\ 13 \end{array} \right\} 58^{\text{s}} \\ \text{P.D.} = 50^{\circ} 20' 45'' \end{array} \right\} \text{R.A. between } 7^{\text{m}} \text{ and } 13^{\text{m}}. \text{ ? the right minute.}$$

The seconds of R.A. should be 48^{s} to correspond with the original observation in the second Catalogue of Herschel, and the R.A. is there stated to be between $16^{\text{h}} 7^{\text{m}}$ and $16^{\text{h}} 12^{\text{m}}$. I find that the correct minute is 12. This is a faint and insignificant pair.

$$\Sigma' (1814) \left\{ \begin{array}{lcl} \text{R.A.} = 16^{\text{h}} 16^{\text{m}} 48^{\text{s}} \\ \text{P.D.} = 88^{\circ} 23' 2'' \end{array} \right\} \begin{array}{l} \text{"Loco } 2041." \text{ Are these really two double} \\ \text{stars? and, if so, to which does the descrip-} \\ \text{tion, position } 4^{\circ} 4', \text{ distance } 3'' \cdot 06, \text{ belong?} \end{array}$$

$$\Sigma\ 2041 \left\{ \begin{array}{lcl} \text{R.A.} = 16^{\text{h}} 16^{\text{m}} 48^{\text{s}} \\ \text{P.D.} = 88^{\circ} 21' 35'' \end{array} \right\}$$

The first number refers to *Positiones Mediæ*, and the place is from that Catalogue. The second place is the same as that in *Mensuræ Micrometricæ*. It is somewhat remarkable that the fact should have been, so far as I am aware, overlooked heretofore that OΣ 308 and Σ 2041 are really the same star. It is a very beautiful pair, and has been measured several times since its discovery. Struve (1831·4) gave $P=4^{\circ}4$; $D=3''\cdot06$; mags. 7·3, 10·5. Mädler (1845·5), measuring it as OΣ 308, found $P=5^{\circ}6$; $D=2''\cdot82$, and Dembowski (1865·8) in his measures of the stars composing Otto Struve's Catalogue gives $P=1^{\circ}9$; $D=2''\cdot61$; mags. 7·2, 9·8. From these results it would seem that the distance must have decreased, while the angle is unchanged. It is worthy of further attention. I have examined very carefully all the other stars in the vicinity, and find only this pair.

Otto Struve's place for 1850 is:

$$\text{R.A.} = 16^{\text{h}} \ 14^{\text{m}} \ 14^{\text{s}} \qquad \text{P.D.} = 88^{\circ} \ 26'$$

I made no special observations for the purpose of identifying this star in the catalogues, beyond ascertaining it was about $12'$ north of σ (50) *Serpentis*. There appears to be confusion in reference to the place. None of the stars in Lalande agree with the foregoing. The nearest are (1850):

	h	m	s	$^{\circ}$	$'$	$''$
L 29860 (8½ mag.)	R.A. = 16	16	44	P.D. = 88	24	1
L 29868 (8 mag.)	R.A. = 16	16	58	P.D. = 88	29	31

I have just referred to Argelander's Catalogue and find a star which agrees with Otto Struve, and is undoubtedly the double in question. It is $11^{\circ}1$ north of 50 *Serpentis* and 17° preceding. Its place for 1850 is:

$$\text{Arg. } (+1^{\circ}) \ 3112 \qquad \text{R.A.} = 16^{\text{h}} \ 14^{\text{m}} \ 13^{\text{s}} \qquad \text{P.D.} = 88^{\circ} \ 26'2$$

Argelander gives the magnitude as 8·2, but it seemed to me rather brighter. It is not in Weisse or Lalande.

$$\text{Mayer} \left\{ \begin{array}{l} \text{R.A.} = 17^{\text{h}} \ 1^{\text{m}} \ 29^{\text{s}} \\ \text{P.D.} = 114^{\circ} \ 2' \ 43'' \end{array} \right\} \text{Is there any double star in this place?}$$

This is 39 *Ophiuchi* (=H III.25=H VI.54=Sh.245). The place does correspond exactly to that given above, but there is no other pair near this place.

$$\Sigma \ 2190 \left\{ \begin{array}{l} \text{R.A.} = 17^{\text{h}} \ 29^{\text{m}} \pm \\ \text{P.D.} = 68^{\circ} \ 53' \end{array} \right\} \left\{ \begin{array}{l} \text{The measures of this star are very conflicting.} \\ \text{Z (1829) makes position } 33^{\circ}22' \text{ h (1830), by a} \\ \text{sweep } 18^{\circ}4' \text{ (plainly written in original, and dia-} \\ \text{gram agreeing) Mädler (1843) } 33^{\circ}67', \text{ and Dem-} \\ \text{bowski (1863) } 23^{\circ}00'. \text{ Which is right?} \end{array} \right.$$

I have not examined this sufficiently to account for the discrepancy positively; but it seems probable that Herschel and Dembowski measured, not $\Sigma 2190$, but $h 2807$, the R.A. of which is exactly the same, but the Decl. is $25'$ less. Struve's measures of his pair are $P=33^{\circ}2$; $D=10''\cdot17$; mags. 6, 9.5. Herschel for $h 2807$ gives $P=22^{\circ}4$; $D=8''\pm$; mags. 7, 11. As will be seen, the two doubles are very similar in magnitudes and distance, with about the difference in angle indicated by the measures given above. I examined this vicinity once, and perhaps twice, and each time failed to see Herschel's pair, notwithstanding the conditions were at least moderately favourable. I am unable to account for the failure to find it, as it should be an easy object, if Herschel's magnitudes are correct. He describes it as "very fine."

$$\text{H I. 60} \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{ccc} h & m & s \\ 18 & 57 & 0 \\ & ' & '' \end{array} \\ \text{P.D.} = 58 \quad 32 \quad 41 \end{array} \right\} \begin{array}{l} \text{Query, as to its existence in this place, or very} \\ \text{near it.} \end{array}$$

The name of this star is printed in the "Queries" H 4599, but that is evidently an error. It might have been written H h 599 as is No. 599 of Sir John Herschel's Catalogue of his father's double stars. The proper designation of the star is as given above. Herschel states in the catalogue last referred to, that this pair may possibly be $\Sigma 2441$ with an error of 5° in the angle. I have shown in a previous paper concerning Sir William Herschel's doublestars (*Monthly Notices*, January 1874), that there is very little if any doubt that these two stars are identical. Herschel II. gave $P=286^{\circ}8$; Struve, $P=291^{\circ}9$. I have carefully examined all the stars in this vicinity, and fail to find any other of Class I; $h 1366$ has the same decl. but is about 3^m preceding. The distance ($10''\pm$) is too great for a double of the class described, and both components are too faint to render it of any special interest. In addition to that, the angle is entirely different ($57^{\circ}8$). I noticed an 8th magnitude star about $2\frac{1}{2}^m$ p. this, with a very minute close double companion, but too faint to be worth cataloguing.

$$\Sigma 2545 \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{ccc} h & m & s \\ 19 & 27 & 32 \\ 19 & 29 & 33 \\ & ' & '' \end{array} \\ \text{P.D.} = 100 \quad 31 \quad 57 \end{array} \right\} \begin{array}{l} \text{Smyth described this as "closely following"} \\ \text{a star R.A. } 19^h 27^m 28^s, \text{ and the R.A.} \\ 19^h 27^m 32^s \text{ given by him differs } 2^m \text{ from} \\ \Sigma. \text{ Which is right?} \end{array}$$

The larger of the two R.A.'s. is right, and is the place given in *Positiones Medice*. This is L 37207, giving exactly the same result. $\Sigma 2541$, $\Sigma 2545$ and $\Sigma 2547$ are in the same neighbourhood, and in some way it is probable Smyth's observations were confused. The identity of this pair with H I. 13, has been clearly shown by Dawes (*Memoirs of the R.A.S.* vol. xxxv), and other evidence bearing upon the same questions has been adduced by Hunt (*Monthly Notices*, January 1872).

Σ (2360)	$\left\{ \begin{array}{lll} \text{R.A.} = 19 & \text{h} & 40 & \text{m} & 4 & \text{s} \\ & \circ & ' & '' & & \end{array} \right.$	$\left\{ \begin{array}{lll} \text{P.D.} = 57 & 31 & 25 \end{array} \right.$	Are these not identical; the P.D. applied to H $\hbar$ 640 being 11' wrong?
H $\hbar$ 640	$\left\{ \begin{array}{lll} \text{R.A.} = 19 & \text{h} & 39 & \text{m} & 54 & \text{s} \\ & \circ & ' & '' & & \end{array} \right.$	$\left\{ \begin{array}{lll} \text{P.D.} = 57 & 19 & 48 \end{array} \right.$	

I have previously called attention to the error in Sir John Herschel's place of this pair (=HN. 110.) This is identical with S. 726. The place is correctly given in *Positiones Mediæ*, as quoted above.

Σ 2634	$\left\{ \begin{array}{lll} \text{R.A.} = 20 & \text{h} & 1 & \text{m} & 50 & \text{s} \\ & \circ & ' & '' & & \end{array} \right.$	$\left\{ \begin{array}{lll} \text{P.D.} = 73 & 41 & 46 \end{array} \right.$	Smyth identifies his No. 733 (<i>Cycle</i>) with this star, and with H $\hbar$ 668; but his P.D. $73^{\circ} 33' 54''$ (for 1830) differs 8'. Are these two double stars?

There is no other pair in or very near this place. This pair (=H II. 70=S. 734) is Weisse XX. 42, 43, corresponding exactly with *Positiones Mediæ*. Smyth's P.D. is erroneous.

σ 680	$\left\{ \begin{array}{lll} \text{R.A.} = 20 & \text{h} & 22 & \text{m} & 53 & \text{s} \\ & \circ & ' & '' & & \end{array} \right.$	$\left\{ \begin{array}{lll} \text{P.D.} = 79 & 23 & 3 \end{array} \right.$	Are not the same double stars? Σ 2690 is identified with Dawes (1); $\hbar$ (269); H $\hbar$ 691; and O Σ 407.
Σ 2690	$\left\{ \begin{array}{lll} \text{R.A.} = 20 & \text{h} & 23 & \text{m} & 6 & \text{s} \\ & \circ & ' & '' & & \end{array} \right.$	$\left\{ \begin{array}{lll} \text{P.D.} = 79 & 18 & 22 \end{array} \right.$	

I cannot find any other double star in or near this. This pair is very much confused in the early catalogues. The corresponding numbers are:—H III. 16=Sh. 325=S. 751= σ 681=O Σ 407=Dawes 1. It is given with measures of the wide pair in South, and Herschel's Catalogue, and again in South's with a different designation, and obviously regarded as a different star. Sir John Herschel included it in his First Catalogue, giving the magnitudes as 9=9, stating "possibly σ 680, but disagreeing much in R.A. and P.D." In a later work he says it is identical with Σ 2690. There are two pairs, in Struve's First Catalogue, σ 680 and σ 681, having the same R.A. with a difference of 10' in decl. The magnitude of the first is given 8, and that agrees in place with L 39521, and is undoubtedly that star. The second (681) is called H III. 16, and the magnitude and decl. correspond with that pair. There is no companion to L 39521—at least near enough to constitute a double star. H III. 16 is erroneously stated by Otto Struve to be $\hbar$ 1521.

$\hbar$ 2994	$\left\{ \begin{array}{lll} \text{R.A.} = 20 & \text{h} & 36 & \text{m} & 18 & \text{s} \\ & \circ & ' & '' & & \end{array} \right.$	$\left\{ \begin{array}{lll} \text{P.D.} = 112 & 7 & 33 \end{array} \right.$	17 <i>Capricorni</i> . "Requires verification." Po- sition $338^{\circ} 7$; distance $20'' \pm$. A very minute companion.

I have examined this star several times when the conditions were sufficiently favorable to show the companion to α^2 *Capricorni* steadily, and I have never been able to see the least trace of the suspected companion. Herschel entered the magnitude as 20; but I have seen many of his minute companions theoretically far beyond the reach of a 6-inch aperture. I am certain this star, if it exist at all, is not within the grasp of my instrument under any circumstances.

$$\begin{array}{l} h\ 1619 \\ h\ 1620 \end{array} \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{ccc} h & m & s \\ 21 & 4 & 22 \\ & 0 & ' & '' \end{array} \\ \text{P.D.} = \begin{array}{ccc} 76 & 9 & 37 \\ 77 & 9 & 36 \end{array} \end{array} \right\} \begin{array}{l} \text{Are not these the same, but } 1^\circ \text{ P.D. mistaken} \\ \text{in one or the other? Which is right? } h\ 1620 \\ \text{is described as quadruple.} \end{array}$$

I examined both places, and failed to find a double in one of them; but from some confusion in the notes, I am unable to state positively in what star the error occurs.

$$h\ 5517 \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{ccc} h & m & s \\ 18 & 14 & 54 \\ & 0 & ' & '' \end{array} \\ \text{P.D.} = 103 \quad 36 \quad 6 \end{array} \right\} \begin{array}{l} 18 \text{ } Aquarii. \text{ Query, a very minute companion at} \\ 13'' \text{ distance.} \end{array}$$

I have never seen the companion, though carefully looked for on several first-class nights. I have found in this general quarter of the heavens a good many new doubles, some of them as difficult as any double stars I have ever seen, and when these were well seen, no companion could be even suspected to 18 *Aquarii*.

$$\begin{array}{l} \Sigma\ 2792 \\ h\ 1638 \end{array} \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{ccc} h & m & s \\ 21 & 15 & 42 \\ & 0 & ' & '' \end{array} \\ \text{P.D.} = \begin{array}{ccc} 61 & 46 & 6 \\ 62 & 46 & 43 \end{array} \end{array} \right\} \begin{array}{l} \text{Query, If not the same, with } 1^\circ \text{ error in P.D.} \\ \text{in one or the other; and if so, which is right?} \end{array}$$

There is an error in Herschel's P.D. beyond doubt. A careful examination of this region leaves but little uncertainty. On this occasion I found a second minute companion to $\Sigma\ 2792$, not noticed by Struve. The distance is rather more than double Struve's star, or about $20''$, and the angle nearly 180° .

$$\begin{array}{l} \Sigma\ 2827 \\ \Sigma\ (2628) \\ \text{" Loco } \Sigma\ 2827 \text{ " } \end{array} \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{ccc} h & m & s \\ 21 & 38 & 54 \\ & 0 & ' & '' \end{array} \\ \text{P.D.} = \begin{array}{ccc} 27 & 9 & 54 \\ 27 & 8 & 39 \end{array} \end{array} \right\} \begin{array}{l} \text{The latter star seems to be only the} \\ \text{former with its place better de-} \\ \text{termined. } \Sigma\ 2827 \text{ is a really ex-} \\ \text{isting double star} = h\ 1690 = \\ \Sigma\ 2827 \text{ of the } Mens. Mic. \text{ Are} \\ \text{these really two double stars very} \\ \text{near each other? } h\ 1690's \text{ place} \\ \text{is } 21^h\ 39^m\ 32^s, 27^\circ\ 11'\ 1''. \end{array}$$

In the "Query" the P.D. of each star is erroneously printed 28° . It should be 27° . I do not find but the one pair in this place. Herschel's place agrees with *Mensura Micrometrica*. I did not observe this pair sufficiently to identify it in the catalogues.

$$h\ 1761 \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{ccc} h & m & s \\ 22 & 16 & 24 \\ \circ & ' & '' \end{array} \\ \text{P.D.} = \begin{array}{ccc} h & m & s \\ 16 & 0 & 38 \\ \circ & ' & '' \end{array} \end{array} \right\} \text{Oblong. Requires re-examination.}$$

Herschel gives $P = 40^\circ \pm$; $D = 1''\frac{1}{2} \pm$; mags. $12 = 12$. I have looked very carefully for this, and in so doing found a new pair (No. 175 of my Third Catalogue, *Monthly Notices*, December 1873), which at first I thought might be Herschel's, but that is not probable, as there is a difference of 12^m in R.A. and $9'$ in Decl. between the two. Of the new pair I estimated, $P = 120^\circ$; $D = 1''\cdot 5$; mags. $9\frac{1}{2}$, $9\frac{1}{2}$. It is an exceedingly difficult object, and could only be well seen under very favourable conditions. This was observed several times, and Herschel's pair also looked for, but without success.

$$h\ 5529 \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{ccc} h & m & s \\ 22 & 28 & 57 \\ \circ & ' & '' \end{array} \\ \text{P.D.} = \begin{array}{ccc} h & m & s \\ 95 & 6 & 4 \\ \circ & ' & '' \end{array} \end{array} \right\} \kappa\ \textit{Aquarii}. \text{ An excessively minute companion strongly suspected.}$$

This star, like 17 *Capricorni* and 13 *Aquarii* has defied all efforts to detect the companion.

$$h\ 3158 \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{ccc} h & m & s \\ 22 & 53 & 15 \\ \circ & ' & '' \end{array} \\ \text{P.D.} = \begin{array}{ccc} h & m & s \\ 20 & 8 & 37 \\ \circ & ' & '' \end{array} \end{array} \right\} \text{Excessively close. Requires verification. Position } 45^\circ.$$

Carefully examined on a good night, but without success. If there is any double here, which I very much doubt, it must be one of extraordinary difficulty.

$$\begin{array}{l} \text{Sh. 358} \\ \sigma\ 790 \end{array} \left\{ \begin{array}{l} \text{R.A.} = \begin{array}{ccc} h & m & s \\ 23 & 46 & 20 \\ \circ & ' & '' \end{array} \\ \text{P.D.} = \begin{array}{ccc} h & m & s \\ 59 & 6 & 20 \\ \circ & ' & '' \end{array} \end{array} \right\} \begin{array}{l} ? \text{ If two distinct double stars. If only one, ? its} \\ \text{place.} \end{array}$$

The second star is called by Herschel $\sigma\ 690$; it should be as given here, $\sigma\ 790$. There is only one pair in this place, and that is Sh. 358: $P = 329^\circ\cdot 2$; $D = 41''\cdot 29$. It is a very uninteresting pair, the stars small and widely separated. This is L 46844-5, from which its place (1830) is:

$$\text{R.A.} = 23^h\ 45^m\ 43^s$$

$$\text{P.D.} = 59^\circ\ 2' 37''.$$

A Fourth Catalogue of 47 New Double Stars, discovered with a 6-inch Alvan Clark Refractor. By S. W. Burnham, Esq.

The double stars forming the following list have been discovered since the observations embraced in the Third Catalogue which appeared in *Monthly Notices* for December 1873. During a considerable portion of this interval the weather has been exceedingly unfavourable, and the number of new objects found is much smaller than it should have been, judging from former results, with a larger and the usual proportion of clear nights.

These stars have all been found with the same instrument previously used, with the single exception of τ *Orionis*, the duplicity of which was discovered with the $18\frac{1}{2}$ -inch refractor of the Dearborn Observatory.

I have measured most of the position-angles; when estimates are made only, the angles have ($\pm$) attached to them. In the absence of a suitable driving clock nothing could be done in measuring distances, and the angular measures have been obtained with more difficulty, and doubtless with less accurate results, in consequence of the want of driving machinery. Many of them, however, are very close, or very unequal and difficult of measurement, even with every advantage.

In the following list the distances of thirty are not more than $2''$, and many of that number are down to $1''$ and less. Several pairs of Struve, Herschel, and others have been found to be again closely double, and some of them are very difficult and interesting objects.

In my last Catalogue, No. 113 was entered as not having been well seen, although there was very little doubt of its being double. Not long since I found this again, independently, and readily recognised it as a double star, but it is an extremely difficult pair.

The places are reduced to 1880. The numbers attached are continued from my former Catalogues in *Monthly Notices* for March, May, and December, 1873.

No. 183 = L 3487.

R.A. = $1^h 47^m 21^s$

Decl. = $- 17^\circ 20'$

P = $240 \pm$

D = $2''$

Mags. $8\frac{1}{2}$, 11.

A pretty pair in *Cetus*, *n f a*, 6th mag. star. Position-angle not measured. Discovered December 9, 1873.

No. 184 = L 8474.

R.A. = $4^h 22^m 34^s$

Decl. = $- 21^\circ 46'$

P = $270^\circ 5$

D = $1''.2$

Mags. 7, 8.

A very elegant pair in *Eridanus*, and not very difficult under favourable conditions. The angle was measured when the star

was not well seen, and is, therefore, not very accurate. December 14, 1873.

No. 185 = L 8745-6.

R.A. = $4^h 31^m 25^s$

Decl. = $-15^\circ 10'$

P = 241°

D = $3''$

Mags. 8, 11.

In *Eridanus*, $38'$ s of 53 *Eridani*, and $1^m 17^s$ f. Easy and rather unimportant pair. February 17, 1874.

No. 186 = L 8986.

R.A. = $4^h 40^m 10^s$

Decl. = $-7^\circ 12'$

P = $180^\circ \pm$

D = $1''.7$

Mags. 8, 10.

A more difficult and much finer pair in the same constellation. No opportunity occurred to measure the angle. December 14, 1873.

No. 187 = L 9397.

R.A. = $4^h 54^m 9^s$

Decl. = $+14^\circ 20'$

P = $270^\circ \pm$

D = $1''$

Mags. 8 = 8.

A close double star on the borders of *Orion* and *Taurus*. It is 2^m following, and $1' 44''$ south of the wide triple, H. V. 113. The latter appears to be identical with H. V. 57 (= Sh. 49 = Σ C.P. 148). The new pair is an interesting object. January 11, 1874.

No. 188 = τ *Orionis*.

R.A. = $5^h 11^m 47^s$

Decl. = $-6^\circ 58'$

(A and D = H.V. 25 = Σ C.P. 167

A and B = H 2259)

A and B, P = $250^\circ 4$

A and D, P = $63^\circ 08$

B and C (new), P = $55^\circ \pm$

D = $18'' \pm$

D = $18'' \pm$

D = $2''$

Mags. 4, 14

Mags. 4, 12

Mags. 14, 16.

The duplicity of the smaller companion to τ *Orionis* was discovered with the Chicago $18\frac{1}{2}$ -in. refractor, December 22, 1873. I was examining the trapezium of *Orion* for the new stars alleged to have been seen within it, and upon directing the instrument to this star saw at once that the companion discovered by Sir John Herschel was double. The second star of the close pair is much smaller, and might easily have escaped attention from an observer not specially interested in double stars. I am not at all certain that the distance is as great as $2''$, and it may not exceed $1''.5$, but, not being very familiar with the eyepieces, my estimate is entitled to less credit than in the other pairs in this list. The measures of B and D, given above, are from Sir John Herschel's Fifth Catalogue of Double Stars, where this companion, as a single star, is first noticed. It is singular that it should have been missed by Herschel I., as with my 6-in. refractor it is very obvious

under almost any circumstances. My instrument, of course, fails to show the close star. I do not know how large an aperture would be necessary, but probably at least 12 or 15 in. would deal with it under the best conditions. The only similar object of the kind in the heavens heretofore observed is α^2 Capricorni. The duplicity of Herschel's 16 mag. companion to that star was discovered by Mr. Alvan Clark, while the 11½-in. refractor was in his possession (and independently, but with the same instrument, by myself at Chicago). Prof. C. A. Young was able to see the companion double, with the 9.4 in. Dartmouth College refractor, on the Rocky Mountain Expedition, at an elevation of about 9,000 feet above the sea. He estimated the distance of the components as 1".5. This agrees nearly with my estimate at the time of finding it. I am not aware of its ever having been seen with any other instruments. Both of these minute double companions should be observed and measured, for there is every appearance of physical connexion, and they may prove as interesting as they are (with ordinary instruments) difficult. I may add here that I could not see the slightest trace of any stars within the trapezium. This is the second time I have examined it with the large telescope under favourable circumstances, when a star of one-tenth the light of the 6th star could not have been overlooked. The failure of this instrument, and the new 26-in. refractor, at the U. S. Naval Observatory, as well as other larger apertures, to show any of the stars claimed to have been seen with much smaller instruments, renders it almost certain that they do not really exist. The theory of variability cannot account for such failure where the object has been so carefully and frequently observed.

No. 189 = *Orionis* 81.

R.A. = 5^h 14^m 32^s

Decl. = - 5° 29'

P = 285° 0

D = 5"

Mags. 7, 15.

This is the *s f* of two bright stars about 1½° n of τ *Orionis*. The companion is excessively minute, and generally it is a very difficult object. When measured, however, it was very well seen, and the measures fairly accordant. The companion seemed to have a reddish tint. February 9, 1874.

No. 190 = *Orionis* 82.

(A and C = Σ 692 = H. IV. 87 = S 475)

R.A. = 5^h 14^m 38^s

Decl. = - 8° 9'

P = 360° $\pm$

D = 0".5

Mags. 8, 8.

The principal star of this wide pair is a very close and exceedingly difficult double. The elongation appeared to be very nearly in the direction of the distant star. The difficulty of the object fully accounts for it being overlooked by Struve, Herschel, South, and others, who have observed and measured A and C.

No change has taken place in that pair. Struve (1831.5) gave $P = 4^{\circ}2$; $D = 34''86$; mags. 8.8. Engelmann (1863.1), $P = 3^{\circ}9$; $D = 35^{\circ}58$. February 9, 1874.

No. 191 = *Arg.* (+ 34°) 1033.

R.A. $5^h 17^m 19^s$ Decl. = + $34^{\circ} 27'$
 $P = 204^{\circ}$ $D = 2''$ Mags. 9, 9.

A not very interesting pair of small stars $2^m 52^s p$ and $7'.8 n$ of $\Sigma 707$. The latter has a second minute companion, not mentioned by Struve. April 12, 1874.

No. 192 = τ *Aurigæ*.

(A and C = H.V. 21 = Σ C.P. 205)

R.A. = $5^h 40^m 52^s$ Decl. = + $39^{\circ} 8'$
 A and B $P = 360^{\circ}$ A and C $P = 32^{\circ}5$
 $D = 25''$ $D = 60''$
 Mags. 5, 14 Mags. 5, 12.

I am not acquainted with any measure of Herschel's companion made heretofore. The new companion is exceedingly faint, and rather less than half the distance of the other. It is too minute for reliable measures. January 4, 1874.

No. 193 = Weisse VI. 208.

R.A. = $6^h 9^m 10^s$ Decl. = + $4^{\circ} 0'$
 $P = 90^{\circ} \pm$ $D = 15''$ Mags. 8, 15.

Another very faint companion, and only seen by careful attention. There is a second brighter companion, $62''$ distant, in the direction of 225° . January 11, 1874.

No. 194 = *Arg.* (+ 38°) 1537.

R.A. = $6^h 28^m 4^s$ Decl. = + $38^{\circ} 6'$
 $P = 283^{\circ}5$ $D = 1''$ Mags. 8, $8\frac{1}{2}$.

A very pretty pair, at first taken to be $\text{O}\Sigma 147$. That was entered in Otto Struve's Catalogue of 1850 with a distance of $0''.6$, with two bright distant companions. Dembowski, in 1865, found the close pair single, as it is now. The star is perfectly round, with a power of 400. The new pair is $1^m 58^s p$ and $4' s$. The three stars in $\text{O}\Sigma 147$ form an equilateral triangle; the side being about $45''$, and the stars being nearly the same brightness, it is a striking object. Possibly the pair I have noted was the one observed by Otto Struve, and in some way confounded with one of the stars of this wide triple. April 2, 1874.

No. 195.

R.A. = $6^h 37^m 7^s$	Decl. = $-23^\circ 8'$
P = 214°	D = $5''$ Mags. 7, 12.

A very unequal pair is *Canis Major*. Measures not very good. March 4, 1874.

No. 196 = Weisse VII. 142.

R.A. = $7^h 6^m 27^s$	Decl. = $-5^\circ 14'$
P = $200^\circ \pm$	D = $14''$ Mags. 9, 10.

The last of a line of three stars in the same field; a rather faint and insignificant object. February 16, 1874.

No. 197 = L 14026.

R.A. = $7^h 7^m 0^s$	Decl. = $-6^\circ 57'$
P = $150^\circ 6$	D = $2''$ Mags. 8, 10.

A pretty pair in *Monoceros*, and readily seen. February 17, 1874.

No. 198 = L 14503.

R.A. = $7^h 20^m 36^s$	Decl. = $-20^\circ 43'$
P = 212°	D = $3''.5$ Mags. 8, 11.

A very fine pair in *Canis Major*. The primary is decidedly red. The pair of 10^m stars $40^\circ p$ and $4' s$ is H 3964. March 4, 1874.

No. 199 = L 14480.

R.A. = $7^h 19^m 57^s$	Decl. = $-20^\circ 56'$
P = 19°	D = $1''.2$ Mags. 7, 9.

An elegant double star just south of H 3964, mentioned in the note to the last. There is a distant 9 mag. companion in the direction of 301° . March 4, 1874.

No. 200 = 70 *Geminorum* = H. VI. 70.

R.A. = 7 ^h 30 ^m 41 ^s	Decl. = + 35° 19'	
A and B, P = 188°·3	A and C, P = 98°	C and D (new), P 249°
D = 98"	D = 160"	D = 1".5
Mags. 6, 11	Mags. 6, 9½	Mags. 10, 12.

Sir William Herschel entered 70 *Geminorum* in his Class VI. of double stars without measures, with estimated distances of each $60'' \pm$. I do not know that these distant companions have ever been measured. The results given above are from my own observations. The most distant of the two companions is a close double of more than ordinary difficulty, and requires very

careful attention, even in good weather, to be well seen. The measures, though made with as much care as possible, may not be very close. February 17, 1874.

No. 201 = L 14945.

R.A. = $7^h 33^m 41^s$	Decl. = $-20^\circ 0'$
P = 330°	D = $2''-4''$
	Mags. 8, 8.

A very easy pair in *Argo*. The distance was not carefully estimated. April 10, 1874.

No. 202.

R.A. = $7^h 56^m 59^s$	Decl. = $-26^\circ 53'$
A and B, P = 162°	A and C, P = 248°
D = $6''$	D = $15''$
Mags. 7, 14.	

A very unequal double, with a more distant companion, C, preceding. March 21, 1874.

No. 203.

R.A. = $7^h 57^m 40^s$	Decl. = $-27^\circ 14'$
P = $246^\circ 5$	D = $5''$
	Mags. 7, 10.

A fine pair in *Argo*. There is a 9 mag. companion $64''$ distant in the direction of $73^\circ 7' (+180^\circ?)$. Herschel has a double, H 4037 (*Cape Obs.*) not far from this place, which I could not find. The angle and distance are both larger than in this pair, so they can hardly be identical. March 22, 1874.

No. 204 = L 16074.

R.A. = $8^h 7^m 2^s$	Decl. = $+10^\circ 45'$
P = $304^\circ 8$	D = $1''$
	Mags. $7\frac{1}{2}$, 11.

An excessively difficult pair, $31' 48''$ s of the binary, Σ 1202. The closeness and great inequality of the components render this a very troublesome object to observe, except under favourable conditions. March 27, 1874.

No. 205 = Pyxis 9 (Bode).

R.A. = $8^h 27^m 54^s$	Decl. = $-24^\circ 7'$
P = $130^\circ \pm$	D = $0''\cdot 5$
	Mags. 7, 7.

Very close and difficult, but finally pretty well seen. An attempt was made to measure it, but the definition was too poor at the time to make the results much better than an estimate. March 12, 1874.

No. 206.

$$\begin{array}{lll} \text{R.A.} = 8^{\text{h}} 30^{\text{m}} 17^{\text{s}} & & \text{Decl.} = - 24^{\circ} 42' \\ \text{P} = 278^{\circ} 6 & \text{D} = 1''.5 & \text{Mags. 8, 9.} \end{array}$$

Another fine pair in the same vicinity as the last. This is the following of two stars nearly in the same parallel, $1^{\text{m}} 22^{\text{s}}$ apart. March 12, 1874.

No. 207 = L 17091.

$$\begin{array}{lll} \text{R.A.} = 8^{\text{h}} 33^{\text{m}} 44^{\text{s}} & & \text{Decl.} = - 19^{\circ} 19' \\ \text{P} = 99^{\circ} & \text{D} = 5'' & \text{Mags. } 6\frac{1}{2}, 11. \end{array}$$

A splendid pair; the primary red, and companion apparently blue. February 16, 1874.

No. 208 = L 17103 = Pyxis 17 (Bode).

$$\begin{array}{lll} \text{R.A.} = 8^{\text{h}} 33^{\text{m}} 54^{\text{s}} & & \text{Decl.} = - 22^{\circ} 16' \\ \text{P} = 30^{\circ} 4 & \text{D} = 1''.4 & \text{Mags. 6, 9.} \end{array}$$

This exquisite pair was found in focussing the glass for No. 205. It is a naked-eye star, and rated 6 mag. in Argelander and Heis. There is a distant companion in the direction of 215° ; $D=72''$. Nos. 205, 206 and this pair are in the same neighbourhood, and were found within a few minutes of each other. March 12, 1874.

No. 209 = Weisse VIII. 849.

$$\begin{array}{lll} \text{R.A.} = 8^{\text{h}} 35^{\text{m}} 24^{\text{s}} & & \text{Decl.} = + 39^{\circ} 14' \\ \text{P} = 355^{\circ} 5 & \text{D} = 1''.6 & \text{Mags. 8, } 8\frac{1}{2}. \end{array}$$

A pretty but very easy pair found in determining the correct place of $\Sigma 1259$, the decl. of which in *Positiones Medice* is $10'$ in error.

No. 210 = L 17696.

$$\begin{array}{lll} \text{R.A.} = 8^{\text{h}} 51^{\text{m}} 18^{\text{s}} & & \text{Decl.} = - 16^{\circ} 58' \\ \text{P} = 182^{\circ} 5 & \text{D} = 2''.3 & \text{Mags. } 7\frac{1}{2}, 7\frac{1}{2}. \end{array}$$

A pretty pair of nearly equal stars. February 16, 1874.

No. 211 = *Hydræ* 68.

$$\begin{array}{lll} \text{R.A.} = 8^{\text{h}} 55^{\text{m}} 42^{\text{s}} & & \text{Decl.} = + 3^{\circ} 9' \\ \text{P} = 255^{\circ} 6 & \text{D} = 1'' & \text{Mags. 7, 10.} \end{array}$$

A splendid, but exceedingly difficult pair, in the same field with $\Sigma 1302$. The components are close and very unequal, and the best circumstances are requisite for a separation of the discs. The measures were apparently good, and accordant. $\Sigma 1302$, an easy pair of small stars, is $43^{\text{s}} p$ and $4' 16'' n$. The new pair is L 17831. April 9, 1874.

No. 212 = *Hydræ* 95.

R.A. = $9^h 10^m 12^s$ Decl. = $-7^\circ 51'$
 P = $218^\circ.5$ D = $1''.5$ Mags. 7, 9.

Another very elegant pair $23' 32''$ n. of 24 *Hydræ*, and 37^s p. The distance not very carefully noted, but measures good. The star is L 18296-7. February 4, 1874.

No. 213 = L 18648.

R.A. = $9^h 22^m 25^s$ Decl. = $-7^\circ 34'$
 P = $185^\circ.5$ D = $1''.5$ Mags. $8\frac{1}{2}$, 10.

This difficult pair is $34' 31''$ north of a *Hydræ*, and is a more difficult object than the last, as both components are much smaller. February 13, 1874.

No. 214 = L 19064 = *Felis* 15 (Bode).

R.A. = $9^h 35^m 52^s$ Decl. = $-17^\circ 56'$
 P = $263^\circ.9$ D = $2''.5$ Mags. $7\frac{1}{2}$, 11.

A very pretty pair, and not difficult. March 12, 1874.

No. 215 = *Lacaille* 4058.

R.A. = $9^h 48^m 41^s$ Decl. = $-27^\circ 26'$
 P = $340^\circ \pm$ D = $1''.5$ Mags. $7\frac{1}{2}$, 11.

An elegant, but excessively unequal and difficult double star. Occasions when so severe a test can be well seen so far south are comparatively rare, and no opportunity occurred after its discovery to obtain a satisfactory measure of the angle. March 26, 1874.

No. 216 = *Lacaille* 4074.

R.A. = $9^h 51^m 20^s$ Decl. = $-25^\circ 59''$
 P = 177° D = $2''.5$ Mags. $6\frac{1}{2}$, 13.

This is one of the most difficult stars of its class in this list. The companion is so minute a point as to require the most careful attention. It is more difficult than the last, which was found a few minutes before on the same evening; but measures were taken at once of this pair, when it was seen much better than at any subsequent time. The places of both are taken from the *Washington Catalogue of Stars*. March 26, 1874.

No. 217.

R.A. = $10^h 1^m 16^s$ Decl. = $-24^\circ 8'$
 P = $273^\circ.5$ D = $1''.5$ Mags. $7\frac{1}{2}$, $7\frac{1}{2}$.

Another very fine pair, found the same evening as the two

preceding. The stars are nearly equal, and being only moderately close, it is much less difficult than the others. Place from the Washington *Mural Circle Zones*. March 26, 1874.

No. 218 = L 19765.

R.A.₂ = $10^h 1^m 40^s$

Decl. = $-19^\circ 7'$

P = $109^\circ 5$

D = $1''$

Mags. 8, 8.

The preceding star of a small equilateral triangle. A beautiful, but difficult pair. March 10, 1874.

No. 219 = *Felis* 54.

R.A. = $10^h 15^m 57^s$

Decl. = $-21^\circ 56'$

P = $193^\circ 6$

D = $2''$

Mags. 7, 9.

Another elegant double in the same neighbourhood. The $10''$ pair of $8\frac{1}{2}$ -in. stars about $4'$ south is H 4303 (*Cape Obs.*) This star in Bode 6 mag. March 10, 1874.

No. 220 = *Crateris* 22.

R.A. = $11^h 6^m 34^s$

Decl. = $-17^\circ 51'$

P = $147^\circ 7$

D = $0''.5$

Mags. 6, 6.

A most elegant double star is *Crater*. It is given as 5 mag. in Bode and Lalande, but 6 mag. in Argelander and Heis. The angle given is a mean of two sets of measures, fairly accordant, considering the closeness of the pair. In Bode this star is designated ψ *Crateris*.

No. 221 = L 24532.

R.A. = $13^h 6^m 54^s$

Decl. = $-14^\circ 49'$

P = $49^\circ 4$

D = $1''.5$

Mags. 8, 10.

This unequal pair is $43'$ north of 53 *Virginis*, and is moderately difficult. March 26, 1874.

No. 222 = L 24636.

R.A. = $13^h 10^m 55^s$

Decl. = $-20^\circ 54'$

P = 10°

D = $1''.5$

Mags. 8, 10.

Another pair in *Hydra*, similar in distance and magnitude to the last. March 26, 1874.

No. 223 = L 25350.

R.A. = $13^h 38^m 58^s$

Decl. = $-2^\circ 43'$

P = $342^\circ 8$

D = $18''$

Mags. 8, 11.

A wide pair, found in looking for a double near the same place, discovered by Winnecke (*Astron. Nach.* 1738). The latter

is a pair of $9\frac{1}{2}$ mag. stars, $4''\cdot68$ apart. The distance seems to be rather less now, and if a rough measure of the angle is not considerably in error, it has diminished since Winnecke's measures in 1855. May 12, 1874.

No. 224 = Weisse XIV. 95.

R.A. = $14^h 7^m 37^s$

Decl. = $+ 13^\circ 8'$

P = 73°

D = $0''\cdot7$

Mags. $8\frac{1}{2}$, $8\frac{1}{2}$.

An excessively difficult pair in *Boötes* $35'$ north of O Σ 269. Measured, but results disagree much. May 6, 1874.

No. 225 = χ Turdi Sol.

(A and B = H.N. 80 = Sh. 179 = Σ C.P. 459)

R.A. = $14^h 7^m 47^s$

Decl. = $- 19^\circ 26'$

A and B (South), P = $295^\circ\cdot8$

B and C (new) P = $103^\circ\cdot4$

D = $35''\cdot12$

D = $1''\cdot5$

Mags. 7, $7\frac{1}{2}$

Mags. $7\frac{1}{2}$, 9.

It is remarkable that the duplicity of B should have escaped the Herschels, South, Jacob, and others, who have observed the wide pair, if it was then as easy as it is now. It could hardly be missed now if examined with any instrument of not less than 4 or 5-inch aperture, under favourable conditions. The wide pair is evidently fixed. Jacob (1847·7) found $P = 295^\circ\cdot7$; $D = 35''\cdot23$. For the same pair I obtained, $P = 295^\circ\cdot8$. The measures of the close pair are very satisfactory, and the result of two nights' observation, the first giving $103^\circ\cdot3$, and the second $103^\circ\cdot5$. This star is L 26319-0. May 12, 1874.

No. 226 = L 26665.

R.A. = $14^h 32^m 4^s$

Decl. = $- 21^\circ 49'$

P = 70°

D = $1''$

Mags. $7\frac{1}{2}$, $7\frac{1}{2}$.

A fine but close and difficult pair in *Scorpio*. May 13, 1874.

No. 227 = B.A.C. 5039.

R.A. = $15^h 12^m 7^s$

Decl. = $- 23^\circ 50'$

P = $184^\circ\cdot1$

D = $1''\cdot7$

Mags. 7, $10\frac{1}{2}$.

This fine pair is the middle one of three stars in the same field, and nearly following each other. This was found in looking for H 4759, a suspected close pair. (See note to next No.) May 6, 1874.

No. 228 = B.A.C. 5041 = H 4756.

R.A. = $15^h 12^m 38^s$

Decl. = $-23^\circ 50'$

P = 329°

D = $0''.8$

Mags. $7\frac{1}{2}, 7\frac{3}{4}$.

Herschel has entered this pair in *Cape Observations* as follows : P = $180^\circ +$; D = $\frac{2}{3}''$; Mags. $9.9\frac{1}{2}$ with the note "Requires verification." On the first examination I took No. 227 to be this star, with a considerable error in Herschel's distance, but upon observing the following star again, I saw at once that it was a close pair, and undoubtedly the one suspected by Herschel, although the magnitudes and position-angle do not correspond very closely, and his decl. is $1'$ too small by the place in B.A.C. and *Washington Catalogue*. It is singular that he should have missed the other pair which is in the same field, $31^s p$. May 12, 1874.

No. 229 = L 45726.

R.A. = $23^h 14^m 26^s$

Decl. = $+56^\circ 35'$

P = 37°

D = $12''$

Mags. 8, 11.

A wide and unimportant pair in *Cassiopeia*. January 15, 1874.

Chicago, 1874, May 12.

Note on the Double Star, $\Sigma 707$. By S. W. Burnham, Esq.

Dembowski, in 1864, in his measures of Struve's double stars, observed this pair, as he supposed, but found two pairs near each other, of which his measures are as follow :—

	⁰	["]		
Preceding in R.A.	P = 51.6	D = 28.62	Mags. 9.5	10.5
Following in R.A.	P = 328.9	D = 20.88	Mags. 9.3	10.0

The measures of $\Sigma 707$ in the *Mensuræ Micrometricæ* are P = $131^\circ 6$; D = $18''.29$; Mags. 8, 10.2. Dembowski has this note :—"On voit deux doubles dans le champ de la lunette, à peu de distance l'une de l'autre. Il y a certainement un fort changement, mais laquelle des deux doubles est la $\Sigma 707$ mesurée par Struve?" (*Astron. Nachr.* 1572). Obviously Struve's pair could not be either of the stars observed; and upon a recent examination I found that Dembowski's pairs are in a low power field with $\Sigma 707$, the following one of the two being about 38^s preceding. These stars are all too faint and wide to have any special interest. I found a second companion to $\Sigma 707$, not noticed by Struve. It is

excessively minute, and would be at least 11.5 mag. of Struve's scale. I estimated the angle at about 60° more than the other companion, and the distance a little more than $5''$ greater, giving approximately, $P = 190^\circ$; $D = 25''$. A single measure of Struve's companion, and Dembowski's following pair, for the purpose of positive identification gave 133° and 328° respectively. The new companion is a difficult object for my 6-inch aperture, but two measures were obtained giving $P = 196^\circ$. This star is not in the *Positiones Medice*. I found that it was identical with Weisse V. 518 (9 mag.)

In examining the neighbouring stars on this occasion I found a much closer pair than either of those mentioned, $2^m 52^s$ preceding, and $7' 8''$ north of $\Sigma 707$. This is *Arg.* (+ 34°) 1033, and given in that catalogue as 8.9 mag., while $\Sigma 707$ is rated 9.1 mag. To me it appeared brighter than either of the stars measured by Dembowski, but a little fainter than Struve's. The distance I estimated as $2''$, and a mean of five measures of the angle gave 202° . In the field a short distance following is a $10''$ pair of very minute stars.

Chicago, 1874, April 24.

Note on the Double Stars, $\Sigma 1801$ and O $\Sigma 265$. By S. W. Burnham, Esq.

Dembowski, in his measures of Struve's double stars (*Astron. Nachr.* 1736) has this note to $\Sigma 1801$: "Aucun satellite—et pour la deuxième fois" (1865.4). A few evenings since I looked this up, and found it without difficulty. It is a faint and insignificant pair, but now a very easy object with my 6-inch refractor. Struve gave $P = 64^\circ 5'$; $D = 18'' 44$; Mags. 9, 10.5. A rough measure of the angle for the purpose of certain identification made it 66° . The magnitudes of the components appeared to be about as estimated by Struve. It would seem that the companion is variable, unless by some accident Dembowski looked in the wrong place. About $2^m 11^s$ following this pair and $4'$ south is a similar but rather brighter pair, which at first I took to be the one in question, with a considerable change in position-angle, but found to be identical with H 2701. Herschel gives no angle, but distance as $15''$. A mean of 3 measures resulted in finding $P = 36^\circ$. The only other observations or mention of $\Sigma 1801$ with which I am acquainted are by Sir John Herschel, who examined it twice, and noted it in his Fourth catalogue, and once in the Fifth catalogue. In the former his magnitudes are 9, 10; in the latter, 9.10, 11.

The double star O $\Sigma 265$ is also noted by Dembowski, "point de

satellite" (1866.3). This is one of the pairs excluded by Otto Struve in the Catalogue of 1850. I see the companion now, although not without some difficulty. In the Catalogue of 1843 the magnitudes are 7, 10, and distance 12". I do not know of any measures of the position-angle. It is very nearly preceding, or about 270° , and is certainly much smaller than 10 of Struve's scale.

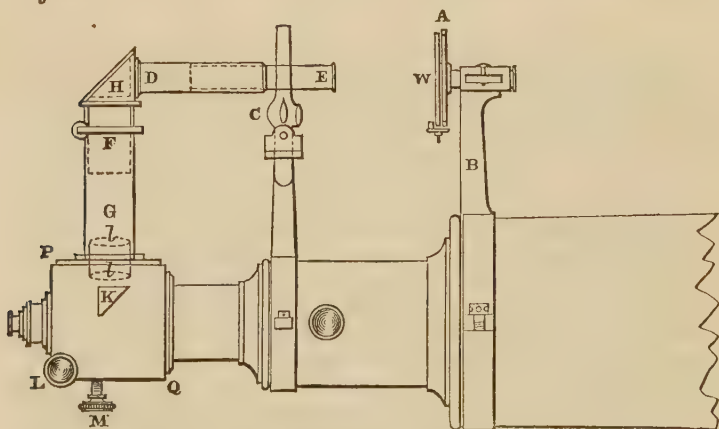
Chicago, 1874, May 17.

On a New Form of Position Micrometer.

By G. P. Bidder, Esq., Q.C.

The accompanying diagram represents a form of position micrometer, designed more particularly for the measurement of faint objects.

The principle of its construction is to throw into the field of view, by means of reflectors, the image of illuminated wires; so producing the appearance of bright wires on a dark field. For purposes of differential measurement, this arrangement affords many advantages over the ordinary dark field micrometer (in which the wires placed in the field are illuminated by side-light), and is, I believe, greatly superior in the measurement of faint objects.



The micrometer, A, which only differs from the common position micrometer in having no eyepiece before the wires, is placed above the telescope, and attached to it by a firm support, B. The wires, W, are illuminated by a lamp, C, in front of them, which is carried on an upright rod attached to the telescope, and is

made capable of being shifted in height and inclination as may be required in order to throw the light on to the wires.

Opposite the wires is a tube, D E, parallel to the telescope, which excludes stray light. A diaphragm at the end nearest the wires reduces the aperture, and assists in effecting the same object.

This tube is attached to a short tube at right angles to it, which fits into the tube F G. This last tube is perpendicular to the axis of the telescope, and is fixed to the square box, P Q, forming a prolongation of the telescope draw-tube. At the intersection of the axis of the two tubes, D E and F G, is placed a rectangular prism, H, and within the tube F G a pair of convex achromatic lenses, *l l*. Below these is a second rectangular prism, K, so placed as to be above the cone of rays passing from the object-glass to any point in the field of view. The light from the wires being thrown by reflexion at the first prism on to the lenses, these form an image of the wires, which, by adjustment of the position of the lenses, is made after reflexion at the lower prism to coincide with the principal focus of the object-glass.

The distance from the wires to the lenses being greater than from the lenses to the focus, the image is reduced in size (in my micrometer the linear dimensions are $\frac{2}{3}$ of the dimensions of the wires themselves). By this means great delicacy of measurement is obtained.

The wires are seen in the field side by side with the stars to be measured, and may be superimposed upon them, but, being mere images, cannot hide them. The light can of course be reduced until the wires are scarcely visible. In the case of excessively faint stars an additional contrivance may be employed, by the help of which the faintest visible objects can be measured. It consists of an opaque bar fixed across a short tube, and inserted at the end E of the tube D E, and as near as conveniently may be to the wires. The bar is placed in such a position as to be transverse to the measuring wires. The effect is to intercept the light from a portion of each wire, and so produce a dark gap in the image of each. The image of the wires being moved in the field so that the stars to be measured appear in these gaps, they can then be readily measured. The lower prism is so mounted that by turning the screw-heads, L M, it can be tilted or turned in any desired direction, by which means the image of the wires may be moved to any part of the field. The whole instrument is so arranged that the screwheads of the micrometer are within reach of the observer while the eye is at the eyepiece.

By the interposition of coloured glass between the lamp and the wires they may be coloured with any desired tint.

It is essential to the accuracy of the measurements that the position of the micrometer and of the lenses, and the distance between them, should not be varied when once adjusted.

I am indebted to Mr. John Browning, who has made my instrument, for the admirable manner in which he has carried out

my design. The instrument, so far as I have hitherto tried it, fully answers my expectations.

A plan somewhat similar to Mr. Bidder's for using a "ghost" of the wires instead of the wires themselves, but applied to a meridional instrument, is described by M. Karl v. Littrow, in the *Proceedings of the Academy of Sciences*, Vienna, Vol. XX., for 1856. M. Littrow uses intercepted wires in the same way as in Mr. Bidder's contrivance.—Ed.

On the Fixing of Spider-lines in Collimators and Transit Telescopes.

By Capt. John Herschel, R.E.

Some time ago I had occasion to replace some broken wires in the diaphragm of a transit telescope. As it is an operation which every practical astronomer ought to be able to perform for himself—more especially when on foreign duty—and as there are difficulties about it which may be lessened by reading an account of a similar operation, of a rather unusual kind, as actually performed, I will endeavour to recall what I can recollect of it.

I may preface this particular account with one or two general remarks upon the subject.

The occasions upon which skill in wiring diaphragms may be brought into play with advantage are so numerous, that it is well to take advantage of every opportunity to put whatever may exist into practice, with a view to prepare for cases of emergency. It does not always happen that the wires of a collimator, for instance, are adapted, in thickness and angle, to the use to which they are to be put—in which case it is very convenient to be able to replace them readily. On foreign service, too, the liability to disfigurement or fracture is much increased; and if confidence has been acquired, a few hours' work at the right time will save much annoyance later. The case I am about to describe was so prefaced, fortunately, by many lesser experiences.

My next remark has reference to ways and means. There are often many ways of doing the same thing, and these often depend on the means at hand. It will be seen that mine were not of an out-of-the-way kind. Such as they were, they sufficed, but possibly others would have served equally well. Some implements, however, are almost essential; for instance, a camel's hair or sable brush—better still, two or three; a lump of beeswax; a few corks, and a few carpentering tools and materials are also useful. Some sort of varnish, too, is necessary—I always use shellac—and some spirits of wine.

The diaphragm upon which I operated was one cut for 25 wires in 5 tallies of 5—the whole occupying about one-third of an inch—on the longer sides of a rectangular plate or frame

about $1\frac{3}{4}$ inch $\times$ $3\frac{1}{2}$ inches. The occasion arose, let us say, after the visit of some obnoxious creature. Whatever it was, the set was damaged, and I concluded to try my hand at a new one.

Now arises the question whether it is better to replace individual wires or the whole set. Considering the relative difficulties—upon which I will not now waste space—I caution anyone against partial reconstruction—especially in delicate cases. At any rate, I decided the question by pushing a finger through the frame.

The next step was to clean the frame. This is very important. Unless all the old fibres and the varnish are removed from the grooves, there will be trouble afterwards. Here the brush and spirit, and a little patience and care, come into play. A cloth may also be applied without fear. It is not an easy thing to scratch brass so deeply with cotton fibre as to form false grooves. Of course the less sand there is in the cloth the better; but there may be a good deal of misplaced tenderness in handling a wire-frame after its wires are broken or condemned.

We proceed now to get the fibre. A good deal might be said about spiders with reference to the fitness of their silk. I do not know enough to speak positively. But I *believe* that very generally what is to all appearance a simple thread is really a bundle of parallel threads. Whether it is possible to get a simple fibre of any considerable length and of sufficient thickness, I doubt. As a fact I am pretty sure that my 25 wires, when finished, were a series of equal strands of fibres—although in appearance as perfect a set as I have ever seen—ininitely superior, I need hardly say, to those which were now beyond the reach of comparison.

The size, uniformity, and quantity of web required are all matters for consideration. The size must be found by trial. Different species of spiders give very different qualities, and in very different degrees. For the sake of uniformity it is very desirable to secure an ample supply from the same source. The quantity will depend on the method of using it.

Having selected my spider, I proceeded to spin from him thus. I had prepared a pane of window-glass with a rod at the back, attached by lumps of wax, to act as a spindle. Having coaxed the spider to attach his web to this, and having shaken him off, I allowed him to *alight*, and then reeled off just fast enough to prevent him from attaching again. I cannot enter into his feelings sufficiently to explain the why and the wherefore, but after many trials I found this the only safe way to get a *long uniform web*, and in this way I generally *could* get an unbroken thread of 30 or 40 feet without stopping. It will be found that very rapid reeling is necessary, and this the 8-inch window-pane spinning on its temporary axle sufficiently provides for. It is hardly necessary to say that, except in a peculiar light, the whole operation is effected without *seeing* the thread. The attitude of the spider alone betrays the connexion; and when, by watching the

creature, the rupture of this connexion becomes known, the direction of the axle must be immediately changed, so that the last two or three turns of the web may cross the glass at an angle—otherwise great difficulty will be experienced in finding the end.

It may seem absurd to wind off so much when only a few inches are wanted. I reply that I wanted 12 or 15 feet, and when my work was done actually had at least 10 feet of continuous thread employed upon the diaphragm.

The next operation introduces a device which is the principal feature in my plan. As yet the thread is wound anyhow. I want to wind it afresh in an orderly manner. For this purpose I make a little hook of light wire, which is to hang on the web as a man hangs with two outstretched arms from a horizontal bar. It is intended to run freely along the web, or the web through it, without risk of the latter twisting upon itself. I will call this the *stirrup*.

The end of the web being found, and one turn unwound, the stirrup is hung on to the slack, and the end attached by a piece of wax to the glass. By this time the need of two Y supports for the spindle will be found, if not already provided. When placed upon them the web will form a swing as it were for the stirrup, and as the spindle is turned the fibre will be wound up on one side as fast as it is unwound at the other, and this winding can be directed by tilting, to any required extent. By this means the whole can be passed under review and re-wound, backwards and forwards, with composure and certainty.

The next step was to transfer the web to the diaphragm. For this purpose I attached a spindle lengthwise at the back of that too, and supported it alongside the other. Then, having transferred one end of the web to the brass frame, the stirrup swinging between, I was able to wind off upon it as much as was wanted, viz. from 30 to 40 turns of the frame, and then, having broken it off from the glass, attached the other end also to the diaphragm at the other end of the rectangle, the stirrup still swinging in the light below. The arranging process was now repeated preliminary to a final re-winding upon the grooves.

This winding and re-winding has the effect of insuring the detection of flaws and of equalising the tension.

The re-winding upon the grooves requires steady supports and a slight tilting arrangement; and the placing of every successive convolution in its place, when the face of the diaphragm is vertical, must be done with the help of a lens held in an appropriate support in front of it.

Ultimately the whole of the 25 convolutions having been thus arranged as nearly as practicable in or close to their respective grooves, the outer edges of the diaphragm were streaked with varnish; and when dry the superfluous fibres at the back cut off, by running a knife-edge along the angles of the frame.

At this stage a final examination of the wires might have

led, but did not, to the substitution of one or more from the unused stock. The perfection of the method is such that it can hardly happen that any such substitution would be necessary.

The last step is the most troublesome. Every fibre has now to be lodged in its groove. This may be done approximately first—always with a brush—and a very weak varnish applied. The obstinate ones can then be manipulated and coaxed with greater freedom. And, lastly, a stronger varnish fixes them all.

I do not know how these things are done by the professional opticians. But I do know that, both in this case and in a second, the intervals between the wires were less unequal after the operation than before. The improvement in the uniformity of the wires as to thickness might be a matter of opinion, but the greater regularity is on record, numerically, as a fact. I infer from this that the method is surer, if not easier.

*Observations of the Total Solar Eclipse of April 16, 1874, at
Klipfontein, Namaqualand, South Africa.*

By E. J. Stone, Esq.

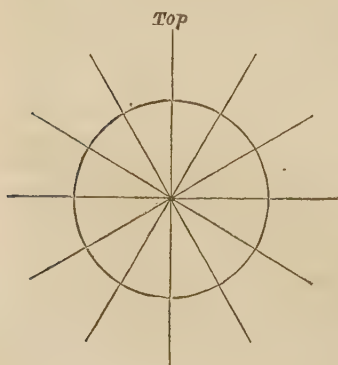
(*Extract from a Letter to the Astronomer Royal.*)

I observed the eclipse from Klipfontein, a station about 3,000 feet above the sea-level. The sky was perfectly clear, and no finer day could have been wished for. I had borrowed a four-inch telescope, mounted as an altazimuth, from Mr. H. Solomon. My spectroscope was one with two dense flint prisms of 60° ; a fair amount of dispersion, therefore, being thus obtained. My great difficulty was to attach the spectroscope firmly to the telescope. Ultimately I was obliged to give up all idea of using the prism of comparison, and to fix the tubes together by wrappers of wash-leather. In this way I secured a very firm connexion between the spectroscope and telescope. I placed two wires in the focus of the telescope of the spectroscope for estimations, and determined to measure only the position of one line in the Corona, the micrometer wire being left untouched until the reappearance of the Fraunhofer lines, when the differences between the line measured and these lines could easily be fixed.

The slit was set as wide as would allow of a clear and distinct view of the Fraunhofer lines. This I did because I expected to find the spectrum of the Corona faint, but was anxious to see whether the Fraunhofer lines were present or not in the spectrum of the Corona. I could not change the width of the slit without taking the spectroscope off the telescope, on account of the way in which I had been compelled to join them together.

During the partial eclipse I most carefully examined the speculum near the Moon's edge, by comparing it with the spectrum away from the Moon's edge, to see whether any fresh

lines were produced by any absorbing medium around the Moon, but I could not find a trace of any difference in the spectra near the Moon's edge and away from it. My slit was so placed that it was parallel to a tangent at the point of last appearance of the sun-light; and as the totality approached, my wife, by the aid of the finder, kept the Sun's limb about half-way across the slit. At the instant of totality the whole field appeared full of bright lines; but I had scarcely time to begin counting these lines before the greater number of them vanished, and the spectrum resolved itself into little more than that of hydrogen gas. Not wishing to spend the few precious moments available upon the spectrum of the prominences, I determined to see what the brightness of the Corona really was before turning the telescope upon it. This was the only view of the eclipse I allowed myself apart from the spectroscope, and I probably spent half a minute lost in admiration of the scene presented to me. The rose-coloured prominences reached very nearly all around the Moon's limb. My wife says there was one, and only one complete apparent break in the continuity. The height varied, of course, very considerably at different parts of the Sun's limb. The Corona was much brighter than I expected to have found it; but its constitution appeared to me uniform throughout, except that its brightness was less as you proceeded further from the Sun. I have a drawing of the Corona by Miss Alice Hall, which in my opinion, and in that of my wife, who observed the eclipse through a pretty good finder, very correctly represents the Corona as seen from our station. I have also a drawing made about 500 miles away, which agrees in all the principal points with that by Miss Hall. I believe that there cannot be a doubt, from these drawings, that the great features of the Corona were identical as seen from these two distant stations. One or two smaller drawings also agree in the principal features, and these drawings are, I think, more to be trusted than usual, from my instructions to refer the prominent points to sectors of 30° having been followed. Diagrams like the rough figure were



prepared, and the line top and bottom adjusted so that it was the section of the paper by a vertical plane as defined by a string with a weight. I believe from the comparisons made, and what I myself saw, that the whole Corona, as seen in South Africa, was a solar appendage. Returning now to my own work—On first moving the telescope away from the prominences, I certainly saw two faint lines less refrangible than E, and one very bright line not far from E; but on moving the telescope more

away from the limb of the Sun I appeared to have lost those fainter lines, and I did not subsequently see them. The spectrum of the Corona appeared to consist of one very bright line and an ordinary sun-light spectrum. I feel perfectly convinced that, although from the faintness of this sun-light spectrum the lines could only be seen with some difficulty, that the Fraunhofer lines were present in the spectrum of the Corona. I examined this point again and again, and I feel more than ever certain upon the point, from my wife's recollection of my strong expression of opinion upon it at the time I was making the examination.

Having carefully looked to this point, I unclamped the spectroscope, and swept over the whole spectrum from the extreme red to the extreme violet; but, independently of the bright line referred to before, I could not at this time see any traces whatever of bright lines. My instrument was then again clamped, and the bright line most carefully bisected by the micrometer wire. I then re-examined the spectrum for Fraunhofer's lines, but without touching the micrometer or moving the telescope even, until the total eclipse was over and the Fraunhofer lines appeared again in all their distinctness. The micrometer was then read, and the coronal line referred to two known lines near it by the micrometer. The wave-length of this bright line agrees so closely with that given by Young, that I could not with my dispersion answer for so small a difference. I am perfectly certain about the numerous lines seen in the spectrum close to the Sun's edge at the commencement of the totality, but the strata giving this spectrum must lie very close to the photosphere, for they were almost immediately covered by the advancing limb of the Moon. I am not prepared to say that the line spectrum of the Corona did consist entirely of one bright line, for, as I have said, I did see three lines near the Sun's limb in the brighter part of the Corona; but I am prepared to say that in the spectrum of the Corona, at some distance from the Sun, and away completely from the red prominences, there was no line in the spectrum of any degree of brightness except the one measured, and that there certainly was in addition to this an ordinary sun-light spectrum with Fraunhofer lines. I presume this spectrum must arise from the reflexion of the sun-light from the gas giving the line spectrum, and that we thus account for the polarisation of the light of the Corona in the plane through the Sun's centre. The natives were much afraid, and went to their huts. They got up a tale that I had brought the eclipse with me, and was looking for a missing star. Independently of the eclipse, I have made magnetical observations at four stations in Namaqualand, one at the Orange River.

Note on Drawings of Jupiter made at Parsonstown, and by M. Terby, at Louvain, in 1873. By the Earl of Rosse.

In the Notes which accompany the chromolithographs of drawings of the planet *Jupiter* made at Parsonstown, which appeared in the *Monthly Notices* for last March, it is suggested that a disagreement as regards position between a marking shown on that of March 7, and a similar one very prominent on M. Terby's of March 8, 6^h 35^m to 6^h 50^m, which we took to be identical with it, might possibly be traced to an error of 1^h in M. Terby's recorded time.

M. Terby having since written to me to say that he feels certain that no such error was made, it seems only due to him that the substance of his remarks should appear in the same publication.

We must now conclude that the feature drawn by M. Terby was exceedingly transient. It does not with certainty appear on a sketch by Mr. N. E. Green, dated March 7, 11^h, which he has been good enough to let me see; and if it can be identified with that which has been seen by us, it must be (as M. Terby suggests) with the marking towards the left of our No. 15, or near the centre of our No. 18.

The repetition of the same feature, with in some cases only exceedingly slight variation, in different parts of the same belt, together with the rapidity with which changes in their shape take place, often renders their certain identification impossible.

Extract from M. Terby's Letter, dated Louvain, May 1, 1874.

"Je puis vous assurer qu'il n'y a pas d'erreur dans le temps de mon observation du 8 Mars, de 6^h 35^m à 6^h 50^m. J'en ai la certitude non seulement par l'inspection de mes notes, mais par le souvenir que, par une circonstance fortuite, j'ai gardé de cette observation. Je me rappelle parfaitement la bande recourbée qui est renfermée dans ce dessin. De plus, cette observation a été faite dans les mêmes conditions que la suivante de 8^h 10^m à 8^h 35^m, que vous avez trouvée exacte, et représentant le crochet dont vous vous occupez. Je vois un autre moyen d'expliquer l'écart que vous signalez et je crois en trouver la confirmation dans vos intéressantes observations elles-mêmes.

"D'après vos calculs, la longitude jovicentrique de la tache en question doit être 260°. Or envisageant votre dessin 3, dans lequel la courbure dont il s'agit occupe la même position que celle de mon dessin 3, je vois que vous avez trouvé pour longitude de votre méridien central 256°.1, et pour le mien 223°.4. Il me semble impossible, d'après cela, d'admettre que les deux dessins représentent la même tache. La bande courbée de mon dessin 3 doit *précéder* celle que vous considérez spécialement, et est devenue invisible dans mon second dessin du 8 Mars, peut-être à cause de sa proximité du bord. Il est bien fâcheux que

jusqu'ici je ne connaisse pas d'observations coïncident avec mon dessin de $6^h\ 35^m$; je suis persuadé que l'on y verrait la bande que j'ai représentée.

"A défaut de coïncidence parfaite, je m'appuierai sur vos dessins 15 et 18, qui renferment des traces évidentes de bandes courbées, précédant celle dont vous vous occupez, car la longitude est ici $200^{\circ}.1$ et $220^{\circ}.6$.

"Ces prolongements établissent une communication entre le bord S de la bande équatoriale et des régions plus septentrionales, semblant avoir été très fréquentes en 1873; on en trouve un autre exemple dans les crochets de mes dessins 2, 9 et 11, que j'avais jugés visibles à peu près sur le même hémisphère, et qui, d'après vous, seraient complètement distincts."

June 8, 1874.

Observations of Jupiter, 1874. By E. B. Knobel, Esq.

Observations of *Jupiter* were commenced on February 8th, 1874, and up to the present time but few opportunities of fair observation have been lost. On the whole the opposition this year has been a very favourable one for study of the planet; a large number of fine nights, coupled with good definition, enables one, I think, to submit one's results with some amount of confidence.

The series of 35 drawings, which I beg to present to the Society, has been made with an $8\frac{1}{2}$ -inch silvered glass reflector by Browning, of excellent quality. The powers used were Browning-achromatic eye-pieces of 144, 208, and 250. Every care was exercised to represent what was actually seen, and to give the relative intensity of the various features; and with a view to obtain as impartial evidence as possible, no comparison of the drawings was made till they were all (with two exceptions) copied out in their present form.

The changes in the planet's aspect since the last opposition have been so numerous, that I have included in the series nearly all the drawings I was able to make. It might be thought that in such a series of drawings, many of which represent the same zenographical meridians, some would be merely replicas of others, but I have found that this is not the case. The state of the atmosphere has made definition variable, and, coupled with absolute changes in the features of the belts, renders it almost impossible for two drawings of *Jupiter* having the same longitude, but executed at different periods, to correspond exactly. Some of the principal features seem to have been permanent for the whole period of observation; others it is difficult to reconcile with what was observed on the same meridian at a subsequent date.

I have arranged the drawings in order of date, but to facilitate comparison, they are given in the following table in order of rotation, the longitude of the centre of the disk being calculated from the tables published in the *Astronomical Register*.*

To eliminate as much as possible discrepancies which may be due only to imperfect definition, I append a column in which the number of crosses indicates *very roughly* the state of the atmosphere, in a scale of 4 degrees, as recommended by Dr. Lohse in his circular of last year :—

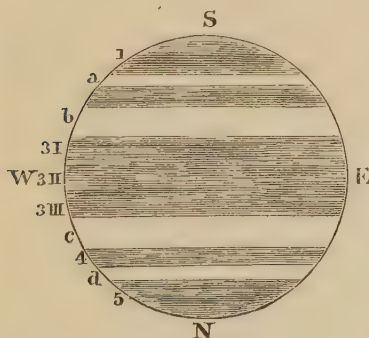
X	=	Definition.	Very good, very sharp and steady.
XX	=	„	Good and steady, but not continuously so.
XXX	=	„	Tremulous; only momentary good views.
XXXX	=	„	Bad, not sharp, though principal features discernible.

No. of Sketch	Date	h m	— Longitude	Definition
20	April 5	8 20 G.M.T.	3	X
17	March 26	10 25 „	12	XX
31	April 29	8 40 „	31	X
6	March 7	10 45 „	41	XX
18	March 31	10 40 „	54	XX
13	March 24	10 15 „	64	XXX
7	March 7	11 25 „	65	XX
34	May 16	8 45 „	74	X
24	April 12	11 0 „	75	XXXX
21	April 5	10 40 „	88	XX
14	March 24	11 0 „	92	XX
25	April 20	8 13 „	99	XX
22	April 8	8 40 „	107	X
8	March 7	13 40 „	147	X
15	March 25	8 30 „	152	XX
23	April 8	10 10 „	162	XX
32	May 2	10 33 „	190	XX
16	March 25	10 0 „	206	X
5	March 6	10 15 „	232	XX
11	March 18	10 25 „	246	X

* It would seem that there is either some error in the rotation period of *Jupiter*, assumed by Mr. Marth in these tables, or else considerable proper motion in the features of the planet. Sketches Nos. 20 and 35 correspond as far as the large white marking in the Equatorial zone is concerned; but the longitude of the centre of No. 20 is $3^{\circ}0'$, and that of No. 35 is $330^{\circ}0'$, showing an apparent error of nearly 1 hour in time, whereas I have every reason to believe there is no serious error in the time of any of the drawings.

No. of Sketch.	Date.	h m	Longitude.	Definition.
4	March 1	(11 45 G.M.T.	253	XX
26	April 21	8 30 "	260	XX
2	February 17	12 30 "	272	X
28	April 23	10 30 "	274	XX
12	March 23	10 30 "	283	XX
29	April 23	11 0 "	292	X
10	March 11	11 10 "	299	X
3	February 17	13 45 "	317	X
30	April 28	11 0 "	325	X
27	April 21	10 18 "	326	XX
33	May 8	9 17 "	329	X
35	June 1	8 57 "	330	XX
19	April 5	7 25 "	330	X
1	February 8	12 20 "	349	X
9	March 9	11 0 "	351	XXX

For the purpose of comparison with the series of chromolithographs of *Jupiter* given by Lord Rosse in the *Monthly Notices* for March, I beg to make use of his diagrams of the belts, in which the dark bands are designated by figures, and the light spaces by letters.



Zone 5. The North Polar zone has appeared about as dark as last year, and in 28 of the sketches it is shown separated from the North Temperate belt 4. In the others no division can be detected between 4 and 5. In seven of the sketches a narrow belt

darker than 5 is shown lying between it and 4. This feature was not observed at all last year. On some nights this narrow belt was seen merely as a thin dark edge to the Polar zone, and it was along this that Satellite III. transitted on March 25.

Zone d has appeared much narrower than last year, and, as mentioned above, on many nights could not be perceived at all.

Zone 4. The North Temperate belt, which has been so conspicuous for the last few years, has appeared on some nights exceedingly faint, and on others singularly darker and more distinct. It has been said to have entirely disappeared this year, but on every occasion my drawings show its existence. Sometimes no separation could be detected between it and 5, but on

those nights it was clearly marked as a broad darker edge to that zone. In sketches 2, 3, 29, and 33 the north edge of this belt is shown decidedly darker than the rest, as observed last year.—*Monthly Notices*, June 1873.

Zone c. The only feature noticed in this, the brightest zone of the planet, was a small projection from the Equatorial zone, shown in sketches 12, 22, and 27.

Zone 3. The Equatorial zone has exhibited many curious changes since the last opposition. The northern portion, 3 III., shows in some drawings a narrow horizontal bright band extending all across the disk; in others there are great breaks and gaps, which sometimes appeared to have no line separating them from c, but these gaps were always observed to be less bright than the adjacent zone. A curious dark spot was noticed on the north edge of 3 III. on April 23rd.

The Equator, 3 II., was on all occasions observed to be marked by a bright band, which contains some very curious bright spaces, referred to hereafter.

The South Tropical belt, 3 I., has appeared invariably darker than 3 III. This portion of the Equatorial zone contains a bright horizontal division, which has not been noticed to be continuous all round the planet; only one sketch between longitudes 70° and 190° gives any indication of its existence between those meridians. In some drawings the northern edge of 3 I. is marked by a much darker line. A curious thinning out of this belt towards the east is observed about longitude 320° . Between longitude 64° and 152° this belt bulges out and widens towards the south by reason of a slant streak from 2, which apparently extends itself along the south edge of § I., and so widens it, but the extra width appeared rather fainter than the rest of 3 I.

Zones 2 and b. The region occupied by these two zones has displayed more evidence than any other of change since last year. In long. 330° a curious curved projection from 2 stretches downwards across b to the Equatorial zone; this has been a permanent feature during the whole period of observation. East and west of this curved projection 2 seems entirely broken up into white cloudy patches and breaks in the continuity of the belt. These white patches have not been noticed between longitudes 40° and 160° . About longitude 40° this South Temperate belt 2 begins to send out to the north-east a slant streak, which continues until it joins the Equatorial zone in longitude 70° . In some drawings of the planet between longitudes 100° and 270° a narrow dark belt is shown running along the centre of b.

Zone a. This bright band has very rarely been seen sharply defined, though it has been detected on most nights of observation. In sketch 31 a narrow dark streak is shown running along the centre of this zone.

Zone 1. The South Polar zone has always appeared fainter than the North.

The colours of *Jupiter* this year have been far more conspicuous than in 1873. A marked change in the tint of the Equatoreal zone has taken place. In May 1873 it was observed of a decided brick red tint; on no occasion this year has that tint been remarked, but a bronze yellow or sienna (mixture of raw and burnt sienna) has prevailed for the whole period of observation, though perhaps on one or two nights the tint approached more of a rich yellow.

The bright band c, immediately north of the Equatoreal zone, has appeared always blue, whether from contrast with the adjacent region or not I cannot say, as what observations I could make with the Equatoreal zone concealed by a bar were unsatisfactory.

One of the most noticeable features in the colours of *Jupiter* is the difference in tint of the North and South Polar zones, already recorded and depicted by Lord Rosse in the interesting series of chromolithographs published in the *Monthly Notices* for March 1874. The South Polar zone has seemed to partake of the tint of the Equatoreal zone in a fainter degree; while the North is, as it has appeared for the last few years, of a bluish grey.

In Mr. Browning's beautiful coloured drawing of *Jupiter*, published in *The Student* for 1869, the dark southern band of the Equatoreal zone is depicted and described as "madder-brown." This particular tint I observed in the same portion of the Equatoreal zone (the South Tropical belt) on several occasions, noticeably on March 31st, at 10.40, when the colours were compared with Mr. Browning's drawing.

I should add that distinct colour views of *Jupiter* seem to go hand in hand with sharp definition, and when the air has been unsteady, colours have been imperfectly seen. With regard to the power necessary to show colour, I must say that a Browning-achromatic eyepiece of 144 has revealed them to me as distinctly and decidedly as any higher power.

The darkest portion of the disk is undoubtedly the South Tropical belt, but this is not uniform in tint, as mentioned above.

The brightest spaces were observed on the Equator in the central light band of the Equatoreal zone. These it has been impossible to represent in the sketches. Particular bright (almost brilliant*) narrow streaks were observed adjacent to the dark South Tropical belt, 3 III. Nothing exactly corresponding is noticed in Lord Rosse's drawings, but somewhat similar light spaces are depicted by Mr. Browning in his drawing before referred to.

* Tacchini describes the appearance in 1873 of certain bright streaks on the Equator of *Jupiter*, as "très vives, comme argentées."

An excellent observation of Mr. Lassell's bright spots on the Southern belts was made on May 17th, at 10.0. I was examining the planet with 144, and made a sketch of it, definition being good. Thinking to improve definition, I put on a $6\frac{1}{2}$ -inch stop, and immediately perceived a brilliant spot like a minute satellite on the south edge of the Equatoreal zone, in zone b. I should say it appeared more brilliant than a satellite, and smaller. Further search enabled me to make out two more spots on the same belt. It is said that these spots are seen only with large apertures; but I have no doubt that reducing the aperture from $8\frac{1}{2}$ inches to $6\frac{1}{2}$ inches first revealed them to me, by materially reducing the brightness of the planet's disk, while these minute spots were brought out in greater contrast; hence they must be of considerable intrinsic brightness. On this evening, with the full aperture, the belts were seen more minutely than with the $6\frac{1}{2}$ -inch stop on, but the bright spots were not nearly so distinct as with the reduced aperture. This fact I satisfied myself about without doubt.

The shadows of the satellites on the disk have been inserted in the drawings as near as possible in the positions observed with regard to the belts. On March 25th the transits of II. and III. with their shadows were observed. At 8.30 III. appeared as a small dusky spot, not round but elongated E. and W., which traversed the edge of the North Polar zone, and a little north of its shadow, which made the transit along the centre of the bright division d. II. and its shadow transitted the North Temperate belt. At 10 o'clock, some minutes before the satellites had begun to protrude beyond the edge of the disk, they were both very distinctly visible as white spots, though when II. was near the centre I could see it only with the greatest difficulty. Precisely the same phenomenon was observed on April 26th, when II. was in transit, and on May 2nd, when Satellite I. was on the disk. On both occasions the satellites were nearly invisible in mid-transit, and on nearing the edge they started into distinct visibility.

The greater distinctness of the satellites themselves when near the limb is a curious phenomenon which has been repeatedly observed by astronomers, but which seems to require explanation. On the occasions mentioned above II. transitted a dark belt, which was most dark near the centre, and fainter towards the limb, yet the satellite was almost invisible when on the darkest part of the belt, and was bright and distinct when the background of the belt was faintest. Satellite III. on March 25th, 1874, appeared as a dark spot when in mid-transit, and on nearing the edge appeared as a bright spot without trace of dusiness. But on March 26th, 1873, Satellite IV. made the whole transit as a dark spot, and was not perceptibly less dark at egress than in mid-transit.

Cloudy weather has prevented any observation of IV. in transit this year. On May 2nd, at 6.40, when this satellite was

on the disk, the planet was picked up in daylight, and though all the belts could be discerned, I could detect nothing of the satellite.

Since writing the above, I have read Mr. Brett's paper on *Jupiter*, in the last number of the *Monthly Notices*, which states that he has detected a dark border upon the white patches on the Equatoreal zone; and this border being always situated on the side furthest from the Sun, he therefore argues that these white patches must be high above the body of the planet and cast shadows.

The feature to which Mr. Brett directs attention is a very curious one, which I do not think has altogether escaped my notice; though as no time is mentioned when this phenomenon was observed by him, I do not know to which particular spots he refers. In the accompanying drawings I have endeavoured to represent similar appearances, if not identically the same; but if so, it is difficult to make them accordant with Mr. Brett's theory.

Jupiter was in opposition to the Sun on March 17th. In sketch No. 14, made on March 24th, at eleven hours, a large white patch or break will be noticed in the northern portion of the Equatoreal zone. The east boundary of this patch was observed much darker than the west, and this darker portion extended for about 5° or 6° of Longitude. Again, in sketch No. 18, made on March 31st, at 10.40, only fourteen days after opposition, the same white patch is shown; and as the dark border east of it was carefully observed that night, I endeavoured to delineate, as accurately as I could, its apparent length (east and west), which I find to be about 8° or 10° of Longitude. Now, it would be easy to calculate what the height of the white patch should be to cast a shadow that length, only fourteen days after opposition. This particular white patch has been observed up to May 16th, and is depicted in two sketches made that evening. In these drawings the dark following space certainly gives a *primâ facie* support to the theory of shadows; but inasmuch as this dark space was observed no longer (east and west) on this evening—two months after opposition—than on March 31st, I must think a true solution of the appearance has hardly been found.

I should add that some of the white patches were not noticed to have any dark border, whilst others, such as those represented in sketches 17, 22 and 30, had dark boundaries on both sides.

Stapenhill, Burton-on-Trent,
1874, June 9.

Third Paper on the Probable Variability of some of the Red Stars in Schjellerup's List (Astr. Nachrichten, No. 1591).

By John Birmingham, Esq.

(Communicated by E. Dunkin, Esq., Hon. Secretary.)

In two former papers laid before the Society I noticed the probable variability of certain stars in the above Catalogue, viz. :—Nos. 63, 77, 90, 91, 101, 152, 241, and 280; and I would now direct attention to Nos. 43, 132, 136, 138, 145, 168, 169, 173, 186, 189, 194, 198, 205, 206, 218, 219, 222, and 247, in each of which I think I have detected evidence of change either in magnitude or colour, or in both. These stars will be generally convenient for observation between this and the next meeting of the Society; and then I hope to communicate my observations of others of the Catalogue that do not well agree with the descriptions transcribed by Schjellerup, but yet require a more extended investigation before one might properly suggest the question of their variability.

I may here state that I have generally determined the magnitudes by estimation, as I have found that, by considerable and careful practice, it is possible in that way to arrive at results which will well bear the test of comparison with the work of "limiting apertures." Between the magnitudes 6 and 10 I believe that I may be considered correct within the limits of a half-magnitude; and, while I feel that, under any circumstances, I could scarcely err to the extent of a whole magnitude, I would remark that this is generally exceeded by the changes which I attribute to the stars under notice.

In judging of colour I am persuaded that in many instances stars appear to me less red than they do to other people; and I should, therefore, be slow to conclude that there must, necessarily, be a change in stars which, though previously described as "ruby," or genuine red, appear to me with a decided tint of orange. However, where the difference between my estimates of colour and those of former observers is very great—such, for instance, as appears in the descriptions of Nos. 168, 173, &c.—the discrepancies must be considered something more than matters of mere personal equation. Nor do I think that differences so striking could be referred to instrumental causes. The refractor with which I have made my observations, though only of $4\frac{1}{2}$ inches aperture, seems very capable of showing the full colour of red stars, as I always find by the variable U *Oygni*, which appears of a deep crimson when even not exceeding the 10th magnitude.

In the following list the numbers refer to Schjellerup's Catalogue, from which I transcribe the descriptions and magnitudes, and then subjoin my own observations with the dates. The positions are for 1872 :—

$$43. \alpha = 4 \overset{h}{43} \overset{m}{30} : \delta = +28 \overset{o}{18} \overset{' }{13} \overset{''}{ }.$$

Cape Observatory, Ruby-coloured. Intense and beautiful colour, 8.

Pale red, 8; 1872, February 10.—Orange red; 1873, January 22.—Good orange red, 7·5; 1874, February 9.—Good red, 7·5; February 15.—Good red, 7·5; February 17.—Good red; scarcely more than 8; February 18.—Colour as usual, 8; February 21.—Good red, 7·8; rather low; February 27.—Fine red, 8; March 6.—Fine red, 8; March 12.—Good red, 7; March 24.—Good red, 8·8-9; April 11.

$$132. \alpha = 10 \overset{h}{31} \overset{m}{15} : \delta = -12 \overset{o}{43} \overset{' }{13} \overset{''}{ }.$$

Hist. Cel. p. 329; rouge. Bessel; roth. Lamont; orange, 6·5.

Fine red with orange cast, 6·6·5; 1873, March 12.—Fine orange star, 5m. at least, and could probably be seen with naked eye only for mist and moonlight; 1874, February 27.—Splendid red orange, 6; March 24.—Fine red orange, 5; April 7.—Splendid red orange, glimpsed occasionally in bad atmosphere with naked eye; May 8.

$$136. \alpha = 10 \overset{h}{45} \overset{m}{24} : \delta = -20 \overset{o}{34} \overset{' }{20} \overset{''}{ }.$$

Hist. Cel. p. 284; bien rouge. Arg.; sehr roth, 6·5.

Fine red, less than 7; 1873, April 20.—Fine red, 8, invisible in finder; 1874, May 8.

$$138. \alpha = 10 \overset{h}{54} \overset{m}{15} : \delta = -17 \overset{o}{38} \overset{' }{14} \overset{''}{ }.$$

Cape Obs. Most intense and curious colour; scarlet, 8.

Very red, 8; 1872, April 13.—Good red, seems under 8; 1873, March 12.—Good red with orange cast, less than 8; April 20.—Genuine ruby, 9-10; 1874, April 7.

$$145. \alpha = 12 \overset{h}{18} \overset{m}{41} : \delta = +1 \overset{o}{28} \overset{' }{44} \overset{''}{ }.$$

Bessel; roth. Lamont, Z 93; rubra. D'Arrest; rubra, 7·5.

A very fine red star, 7; 1872, April 15.—Fine red, 6·6·5; 1874, February 27.—Fine deep orange red, 6·6·5, April 10 and May 8 and 9.

$$168. \alpha = 14 \overset{h}{18} \overset{m}{0} : \delta = +8 \overset{o}{40} \overset{' }{14} \overset{''}{ }.$$

D'Arrest; fulva et quidem egregie, 6.

No tinge of red; 1872, April 15 and 27.—A tinge of very palest orange; May 9.—Perhaps a slight yellow cast; 1873, March 14.—Certainly not red; light straw colour, 6·5; April 21, —No certain colour; April 23.—No striking colour; perhaps light straw colour, 7; 1874, April 10.—Rather reddish, contrasted with a blue between it and 96 *Boötis*, and smaller than the blue; May 7.—Rather reddish, 6·6·5; June 7.

$$169. \alpha = 14^{\text{h}} 18^{\text{m}} 25^{\text{s}} : \delta = +26^{\circ} 17' 14''.$$

Cape Obs. vivid red; almost bright ruby, 7.5.

Fine red orange, 7.3; 1872, April 7.—Fine red, 9; 1874, April 10.—Good red, 7.5; June 7.

$$173. \alpha = 14^{\text{h}} 29^{\text{m}} 25^{\text{s}} : \delta = +37^{\circ} 11' 21''.$$

Hist. Cel. p. 164; rouge, 6.

Faint yellow tinge, 1872, April 27.—Faint straw colour, if any, 7; 1874, April 10.—Faint straw colour, 6; May 9.—Faint straw colour, 6-6.5; June 7.

$$186. \alpha = 16^{\text{h}} 3^{\text{m}} 11^{\text{s}} : \delta = +1^{\circ} 9' 40''.$$

D'Arrest; subrubra, 8.

Reddish, 7; 1872, April 27.—Pale reddish, 8; 1873, March 9.—Reddish, 7.5-8; 1874, April 10.—Reddish, 7; May 7.—Dull reddish tinge, 7-7.5; June 7.

$$189. \alpha = 16^{\text{h}} 19^{\text{m}} 37^{\text{s}} : \delta = -12^{\circ} 7' 34''.$$

Cape Obs. dull brick red, 8.

Good red, 9-9.5; 1873, April 6.—Genuine ruby, 9; 1874, April 10.—Fine red, 7.5; May 8.

$$194. \alpha = 16^{\text{h}} 44^{\text{m}} 35^{\text{s}} : \delta = -5^{\circ} 57' 19''.$$

Schj. 5991; roth, 8.

Not seen; 1872, May 9.—Not red, 9.5; 1873, April 6 and 20.—Seems less than 9, and pale reddish; April 23.—Perhaps reddish, 10-11; 1874, April 10.—Reddish, 10 at most; May 8 and 9.

$$198. \alpha = 16^{\text{h}} 53^{\text{m}} 4^{\text{s}} : \delta = -4^{\circ} 1' 36''.$$

Schj. 6046; gelbroth, 8.

Slight reddish tinge [30 *Ophiuchi*. Near it is a fine amber-coloured star with orange cast] 1873, April 6.—Slightly, if at all, red, 9.5; 1874, April 10.—Slightly, if at all, reddish, 9; May 8.

$$205. \alpha = 17^{\text{h}} 37^{\text{m}} 25^{\text{s}} : \delta = -18^{\circ} 35' 51''.$$

Cape Obs. Remarkable ruby red, 8.

Slightly red, not more than 9; 1873, May 18.—Red not deep, 9-9.5; 1874, April 10.—Reddish, 8-9; May 7.

$$206. \alpha = 17^{\text{h}} 47^{\text{m}} 23^{\text{s}} : \delta = +1^{\circ} 47' 39''.$$

Cape Obs. A curious ruby-coloured star, 9.

Tinged with red, 7-8; 1871, September 4.—Not seen; 1872,

May 9.—Not seen; 1873, April 20.—Not seen (strong twilight); 1874, April 10.—Not seen (fine sky); May 8.—The Rev. Mr. Webb has independently remarked the variability of this star.

$$218. \alpha = \begin{matrix} h & m & s \\ 18 & 39 & 34 \end{matrix} : \delta = + \begin{matrix} ^{\circ} & ' & '' \\ 8 & 37 & 1. \end{matrix}$$

Cape Obs. Plum-coloured, or ruddy purple, 9.

Not seen; 1872, May 11.—Not seen; 1874, May 8.

$$219. \alpha = \begin{matrix} h & m & s \\ 18 & 42 & 52 \end{matrix} : \delta = - \begin{matrix} ^{\circ} & ' & '' \\ 8 & 2 & 58. \end{matrix}$$

Cape Obs. Most remarkable ruby red, 9.

Fine orange red, 7.5-8; 1873, April 18.—Very fine orange red, 7-8; September 23.—Fine orange red, 8 at least; 1874, May 8.

$$222. \alpha = \begin{matrix} h & m & s \\ 18 & 52 & 42 \end{matrix} : \delta = + \begin{matrix} ^{\circ} & ' & '' \\ 14 & 11 & 9. \end{matrix}$$

Lamont, Z 199; rubra, 8.

Fine red, 8; 1872, May 16.—A good red, 9-9.5; 1873, April 6.—Fine red, 9-5.10; 1874, May 8. Secchi says of this star in his "Memoria II.," presented to the Italian Society in November 1868: "È piccola di 9^a, e nulla ha di particolare nello spettro." He gives no date of observation.

$$247. \alpha = \begin{matrix} h & m & s \\ 21 & 9 & 34 \end{matrix} : \delta = + \begin{matrix} ^{\circ} & ' & '' \\ 59 & 35 & 12. \end{matrix}$$

Conn. d. T. xv.; rouge. Arg.; sehr roth, 8.

Red, not deep, 6.5; 1872, July 14.—Orange red, 6.5; 1873, September 28. Secchi, in the Memoir already quoted, says of this star: "Debolmente rossa di 8, 9, senza nulla di singolare nello spettro."

Phenomena of Jupiter's Satellites observed at the Stonyhurst Observatory in 1873-74. By the Rev. S. J. Perry.

Date. 1873.	Satellite.	Phenomenon.	G.M.T.	Observer.	Remarks.
			$\begin{matrix} h & m & s \end{matrix}$		
May 2	II.	Sh. I. internal contact	8 58 18.3	S. P.	Probably late.
		Tr. E. int. contact	9 9 56.3	"	
		bisection	9 14 12.8	"	

Date. 1873.	Satellite.	Phenomenon.	G.M.T. h m s	Observer.	Remarks.
May 9		Tr. I. external contact	8 54 27.5	W. C.	
		bisection	8 57 6.5	"	
		int. contact	8 59 52.0	"	Thin clouds.
May 11		Ecc. R. first seen	9 30 1.6	S. P.	} Very good.
		full brightness	9 32 26.8	"	
May 12	I.	Occ. D. last seen	11 48 28.4	"	Clouds passing.
May 13		Tr. I. ext. contact	9 7 58.2	"	} Slight mist. Definition not good.
		bisection	9 10 27.5	"	
		int. contact	9 12 42.5	"	
		Sh. I. bisection	10 28 40.5	"	Probably late.
		int. contact	10 30 14.0	"	
		Tr. E. int. contact	11 25 40.2	"	} Mist, very bad.
		bisection	11 28 51.5	"	
		ext. contact	11 31 33.0	"	
May 14		Ecc. R. first seen	9 51 12.6	"	Slight haze.
May 21	III.	Sh. E. bisection	8 47 51.3	W. C.	} Very good. Shadow very dark.
		last seen	8 49 39.8	"	
May 22	I.	Sh. E. int. contact	9 3 53.1	"	} Faint. Boiling.
		bisection	9 5 19.6	"	
		last seen	9 7 2.6	"	
	IV.	Ecc. R. first seen	10 44 12.6	"	
May 28	III.	Sh. I. bisection	9 24 1.6	S. P.	
		int. contact	9 26 47.1	"	
	I.	Occ. D. first contact	10 13 9.0	"	
		bisection	10 15 27.1	"	
June 4	III.	Tr. I. ext. contact	8 21 33.1	"	} Daylight.
		bisection	8 25 10.6	"	
		int. contact	8 28 18.6	"	
Oct. 19	III.	Sh. I. bisection	16 53 38.6	W. C.	Not very good.
Oct. 28	I.	Ecc. D. last seen	16 30 21.9	"	
Nov. 12	I.	Tr. I. ext. contact	18 17 27.5	"	
		bisection	18 20 2.0	"	
		int. contact	18 22 51.5	"	
Nov. 25	IV.	Ecc. R. first seen	16 2 13.1	"	
Dec. 11	III.	Occ. D. first cont.	15 47 36.0	"	
		bisection	15 51 22.5	"	
		last seen	15 54 20.0	"	
1874. Feb. 3	III.	Tr. E. bisection	10 53 58.1	"	} Clouds passing.
		ext. contact	11 0 21.1	"	

Date. 1874.	Satellite.	Phenomenon.	G.M.T.			Observer.	Remarks.
			h	m	s		
Feb. 3	II.	Sh. I. int. contact	14	22	30.1	W.C.	Very faint.
		Tr. I. first contact	15	53	17.1	"	
		bisection	15	56	18.6	"	
		int. contact	15	59	34.1	"	
		Sh. E. int. contact	16	43	35.1	"	Shadow very dark.
		bisection	16	46	18.1	"	
		last seen	16	48	55.1	"	
		Tr. E. int. contact	18	22	5.1	"	
		bisection	18	28	8.1	"	} Tremulous. Thin clouds.
		ext. contact	18	33	44.1	"	
	I.	Ecc. D. light fading	18	50	20.1	"	
		last seen	18	53	46.1	"	
Mar. 9	II.	Ecc. D. last seen	8	14	37.5	"	
Mar. 18	III.	Tr. E. bisection	7	1	11.1	"	
		last contact	7	6	13.1	"	
	II.	Tr. E. bisection	8	3	7.1	"	
		ext. contact	8	8	20.1	"	
	I.	Occ. R. bisection	8	27	58.1	"	
		last contact	8	31	39.1	"	
Mar. 25	II.	Tr. I. bisection	7	40	24.68	"	
		last contact	7	44	35.2	"	
	III.	Sh. E. first contact	11	2	18.2	"	
		bisection	11	6	20.7	"	
		last contact	11	10	30.7	"	
May 17	I.	Ecc. R. first seen	12	37	4.1	"	
	II.	Tr. I. ext. contact	12	4	54.6	"	
		bisection	12	59	44.5	"	
		int. contact	13	4	45.6	"	

The above observations were made during the twelve months from May 1873 to May 1874. Most of the observations in 1873, and all in 1874, were taken by Mr. W. Carlisle. The instrument used was the 8-inch Equatoreal.

*Stonyhurst Observatory,
1874, June 12.*

Observations of Occultations of Stars by the Moon, 1873 and 1874, (with the deduced Equations between the Errors of the Lunar Elements); and of Phenomena of Jupiter's Satellites in 1874: made at the Radcliffe Observatory, Oxford.

(Communicated by the Radcliffe Observer.)

Occultations.

No.	Day of Observation.	Phenomenon.	Moon's Limb.	Oxford Mean Solar Time.			Observer.
				h	m	s	
1	1873. July 4	Dis. of λ Virginis	Dark	10	9	9.3	M & K
2	Oct. 3	„ τ^1 Aquarii	„	6	26	22.5	K
3	Nov. 5	„ A^1 Tauri	Bright	11	56	57.1	L
4	Dec. 24	„ τ^2 Aquarii	Dark	4	24	34.3	M & K
5	1874. Jan. 25	Dis. of 53 Arietis	Dark	11	43	46.9	K
6	„ 26	„ A^1 Tauri	„	12	30	20.9	L
7	„ 27	„ k Tauri	„	10	34	13.0	K
8	„ 30	„ c Geminorum	„	6	0	16.1	K
9	Mar. 26	„ λ Cancri	„	11	41	34.6	K
10	Apr. 22	„ ω^1 Cancri	„	9	39	35.7	K
11	„	„ ω^2 Cancri	„	10	6	20.0	M & K

Notes.

1873. July 4. The disappearance was instantaneous and the observation good. The mean of the observed seconds 9.0 and 9.5 has been taken.
 Oct. 3. The observation good.
 Nov. 5. The Moon and star rather unsteady; thin cloud.
 Dec. 24. Very exact (M.). Very good (K.). The mean of the observed seconds 34.5 and 34.2 has been taken.
1874. Jan. 25, Jan. 26, Jan. 27, Jan. 30, Mar. 26. The disappearances were instantaneous and the observations good.
 Apr. 22. (Disapp. of ω^2 Cancri.) Very good (M.). The mean of the observed seconds 19.8 and 20.2 has been taken.
 „ (Disapp. of ω^1 and ω^2 Cancri.) Disappearances instantaneous and the observations good (K.).

In the following table of the errors of lunar elements resulting from the occultations the Greenwich notation is used, and the elements of the *Nautical Almanac* are used uncorrected. All the computations have been made by Mr. Main by the method given in his treatise on *Spherical and Practical Astronomy*.

The observations are referred to by the Nos. of reference given above.

$$1, + 6.42 = + 0.378 \times e + 0.922 \times f - 0.378 \times x - 0.923 \times y - 0.320 \times t \\ - 2.144 \times m - 0.916 \times n.$$

- 2, + 9'48 = + 0'960 × *e* - 0'151 × *f* - 0'960 × *x* + 0'150 × *y* - 0'494 × *t*
 - 1'272 × *m* - 0'996 × *n*.
- 3, + 4'38 = + 0'146 × *e* - 0'988 × *f* - 0'146 × *x* + 0'988 × *y* - 0'248 × *t*
 + 1'675 × *m* - 0'967 × *n*.
- 4, + 8'97 = + 0'973 × *e* + 0'016 × *f* - 0'973 × *x* - 0'018 × *y* - 0'374 × *t*
 - 0'104 × *m* - 0'971 × *n*.
- 5, + 8'73 = + 0'578 × *e* - 0'794 × *f* - 0'578 × *x* + 0'794 × *y* - 0'427 × *t*
 + 3'223 × *m* - 0'954 × *n*.
- 6, + 8'47 = + 0'924 × *e* + 0'052 × *f* - 0'924 × *x* - 0'051 × *y* - 0'491 × *t*
 + 1'948 × *m* - 0'945 × *n*.
- 7, + 8'77 = + 0'899 × *e* - 0'104 × *f* - 0'899 × *x* + 0'107 × *y* - 0'416 × *t*
 + 1'328 × *m* - 0'937 × *n*.
- 8, + 5'50 = + 0'657 × *e* - 0'678 × *f* - 0'657 × *x* + 0'679 × *y* - 0'353 × *t*
 - 0'045 × *m* - 0'915 × *n*.
- 9, + 6'15 = + 0'367 × *e* + 0'914 × *f* - 0'367 × *x* - 0'914 × *y* - 0'319 × *t*
 - 1'035 × *m* - 0'908 × *n*.
- 10, + 7'83 = + 0'825 × *e* - 0'392 × *f* - 0'825 × *x* + 0'394 × *y* - 0'345 × *t*
 + 2'361 × *m* - 0'922 × *n*.
- 11, + 6'30 = + 0'766 × *e* + 0'524 × *f* - 0'766 × *x* - 0'522 × *y* - 0'469 × *t*
 + 0'562 × *m* - 0'922 × *n*.

Phenomena of Jupiter's Satellites.

Day of Obs.	Satellite.	Phenomena.	Instrument and Power used.	Oxford Mean Solar Time of Observation. h m s	Greenwich Mean Solar Time from N. A. h m s	Obser- ver.
1874. Jan. 30.	I.	Tr. egr. bisection	Helium.	11 45 4'6	11 51	B
		„ last contact	with power 150	11 46 49'3		
	IV.	Ecl. disappearance	„	12 23 23'6	12 32 10'0	K
Mar. 7.	III.	Ecl. disappearance	„	10 23 24'0	10 24 23'0	„
„ 9.	II.	Occ. reap. first app.	10-ft. tel.	11 7 32'4	11 14	„
		„ last contact	with power 150	11 12 31'6		
	I.	Occ. reap. bisection	„	12 12 51'9	12 18	„
		„ last contact	„	12 14 51'6		
„ 18.	II.	Tr. egr. first contact	Helium.	7 55 59'0	8 4	„
		„ bisection	with power 200	7 57 58'7		
		„ last contact	„	8 0 28'2		

Day of Obs.	Satellite.	Phenomena.	Instrument and Power used.	Oxford Mean Solar Time of Observation. h m s	Greenwich Mean Solar Time from N.A. h m s	Obser- ver.
1874.						
	I.	Occ. reap. first app.	Helium.	8 20 54.8		
		" bisection	with power	8 22 24.6	8 28	K
		" last contact	200	8 24 24.3		
Mar. 24.	I.	Tr. ingr. first contact	10-ft. tel.	10 34 1.3		
		" last contact	with power	10 39 10.4	10 43	L
		" first contact	Helium.	10 33 8.2		
		" bisection	with power	10 35 17.8	10 43	B
		" last contact	"	10 38 47.2		
" 25.	III.	Tr. ingr. first contact	"	7 11 25.7		
		" bisection	"	7 14 55.1	7 21	K
		" last contact	"	7 19 24.5		
	II.	" first contact	"	7 34 22.0		
		" bisection	"	7 37 21.5	7 41	"
		" last contact	"	7 40 36.0		
	I.	Occ. disap. first contact	"	7 48 49.6		
		" bisection	"	7 50 49.3	7 56	"
		" last contact	"	7 52 49.0		
	II.	Tr. egr. first contact	"	10 7 56.8		
		" bisection	"	10 10 26.4	10 17	"
		" last contact	"	10 13 25.9		
	III.	Tr. egr. first contact	"	10 7 56.9		
		" bisection	"	10 11 56.2	10 22	"
		" last contact	"	10 15 55.5		
	I.	Ecl. reap. first seen	"	10 16 10.5		
		" full brightness	"	10 19 24.9	10 21 2.5	"
	II.	Sh. egr. first contact	"	10 30 23.1	10 43	"
	III.	Sh. egr. first contact	"	11 1 48.0		
		" bisection	"	11 5 47.3	11 15	"
		" last contact	"	11 8 46.8		
" 26.	I.	Tr. egr. first contact	"	7 16 28.6		
		" bisection	"	7 18 28.3	7 24	"
		" last contact	"	7 20 28.0		
" 31.	I.	Tr. ingr. first contact	10-ft. tel.	12 20 25.0		
		" bisection	with power	12 22 24.7	12 27	L
		" last contact	150	12 25 4.3		

Day of Obs.	Satellite.	Phenomena.	Instrument and Power used.	Oxford Mean Solar Time of Observation. h m s	Greenwich Mean Solar Time from N.A. h m s	Obser- ver.
1874.						
Apr. 8.	I.	Sh. ingr. last contact	10-ft. tel.	12 47 07	12 47	L
	I.	Occ. dis. first contact	with power 150	11 17 14.0	11 24	"
		" bisection	"	11 19 23.7		
		" last contact	"	11 20 43.5		
	II.	Tr. ingr. first contact	"	12 2 13.1	12 8	"
		" bisection	"	12 5 12.6		
		" last contact	"	12 9 31.9		
" 10.	I.	Ecl. reappearance	"	8 32 20.2	8 37 26.0	"
	II.	Ecl. reap. first seen	"	10 30 2.3	10 36 15.4	"
		" fully seen	"	10 30 22.3		
	I.	Ecl. reappearance	Heliom.	8 32 20.2	8 37 26.0	B
	II.	Ecl. reappearance	with power 200	10 30 2.3	10 36 15.4	"
" 24.	I.	Occ. dis. first contact	10-ft. tel.	9 14 51.2	9 22	L
		" last contact	with power 150	9 19 30.4		
	II.	Occ. dis. first contact	"	11 21 18.9	11 30	"
		" last contact	"	11 25 58.1		
" 25.	I.	Tr. egr. first contact	Heliom.	8 43 36.3	8 55	K
		" bisection	with power 200	8 45 36.0		
		" last contact	"	8 48 35.5		
May. 2.	I.	Tr. ingr. first contact	"	8 19 42.4	8 26	B
		" bisection	"	8 22 12.0		
		" last contact	"	8 25 41.4		
	I.	Sh. ingr. first seen	"	9 22 47.1	9 24	K
	I.	Tr. egr. bisection	"	10 39 19.5	10 43	"
		" last contact	"	10 41 19.2		
" 7.	III.	Sh. egr. last seen	10-ft. tel.	10 57 37.0	11 5	"
" 12.	II.	Ecl. reap. first seen	with power 150	10 15 34.9	10 21 54.5	L
		" fully seen	"	10 16 12.8		
" 16.	I.	Tr. ingr. first contact	Heliom.	12 0 6.1	12 6	K
		" bisection	with power 200	12 2 35.7		
		" last contact	"	12 5 35.2		
" 18.	I.	Tr. egr. first contact	"	8 43 16.8	8 51	"
		" bisection	"	8 45 16.5		
		" last contact	"	8 47 16.2		

Day of Obs.	Satellite.	Phenomena.	Instrument and Power used.	Oxford Mean Solar Time of Observation. h m s	Greenwich Mean Solar Time from N.A. h m s	Obser- ver.
1874.						
June 1.	III.	Ecl. dis. Began to fade in brightness	10-ft. tel.	10 3 6.0	10 9 47.3	K
		" final dis.	with power 150	10 8 25.1		
	I.	Tr. ingr. first contact	"	10 10 4.9		
		" bisection	"	10 13 4.4	10 18	"
		" last contact	"	10 16 3.9		
" 4.	II.	Tr. egr. first contact	Heliom.	9 52 55.9	9 56	"
		" bisection	with power 200	9 55 25.5		
		" last contact	"	9 57 55.1		
" 8.	III.	Occ. dis. first contact	10-ft. tel.	8 43 14.6	8 50	"
		" bisection	with power 150	8 47 13.9		
		" last contact	"	8 51 13.2		
" 9.	I.	Occ. dis. first contact	"	9 13 35.1	9 21	L
		" last contact	"	9 17 24.5		
" 11.	II.	Tr. egr. first contact	"	9 42 44.7	9 48	K
		" bisection	"	9 45 44.2		
		" last contact	"	9 49 43.5		
" 13.	II.	Ecl. reap. first seen	Heliom. with power 200	10 1 54.0	10 7 36.3	"
		" full brightness		10 4 8.6		

Notes.

- Jan. 30. J. IV. Ecl. disapp. *Jupiter* and the satellites were very well defined. The satellite faded in brightness to a very small point of light, and disappeared suddenly at the time noted.
- Mar. 7. The observation difficult, as the satellite disappeared close to the planet, and, before disappearing, became an extremely faint point of light.
- " 9. J. II. Occ. reapp. Doubtful; cloudy. J. I. Occ. reapp. The observation good.
- " 24. The planet was well defined and steady. (B.)
- " 25. J. II. and III. Trs. egress, and J. I. Ecl. reapp. The planet very tremulous.
- " 26. The planet very tremulous.
- Apr. 24. Cloudy at both phenomena.
- " 25. The planet tremulous.
- May 2. J. I. Trs. ingress. The planet very tremulous. J. I. Sh. Ingress. The shadow was well on the disc when "first seen."
- " 7. The sky became cloudy immediately after the time noted for "last seen."
- " 16. The image of the planet bad.
- June 1. J. III. Ecl. disapp. The satellite became the merest point of light before finally disappearing.
- " 4. The image of planet bad.

9. The planet and satellite unsteady.
 11. The planet tremulous.

The initials M., L., K., B., are those of Mr. Main, Mr. Lucas, Mr. Keating, and Mr. Bellamy.

The instruments used were the Heliometer and the 10-foot telescope.
 The assumed longitude of the Radcliffe Observatory is $5^m 2^s 6$ West.

Occultations of Stars by the Moon, and Eclipses of Jupiter's Satellites, observed at Windsor, New South Wales. By John Tebbutt, Esq.

The following is a list of lunar occultations of stars and eclipses of *Jupiter's* satellites observed by me during the year 1873. The occultations are disappearances only at the Moon's dark limb, the phenomena not being previously computed. The quadrant of the moon's limb, where it could be determined, at which each disappearance took place, will be found in the fourth column of the occultation table. Where the quadrant is not stated, it is to be understood, except in the case of May 12th, that the disappearances occurred in or very near the plane passing through the observer and the centres of the Sun and Moon. The second occultation for April 1st was observed with the old $3\frac{1}{4}$ -inch refractor, and a power of about 30; all the other occultations were observed with the $4\frac{1}{2}$ -inch Equatoreal, and a power of 55. The latter instrument, with a power of 180, was employed in all the eclipse observations.

Lunar Occultations of Stars, 1873.

Date of Obs.	Mag. of Star.	Observatory Mean Time of Disappearance.			Quadrant.	Remarks.
		h	m	s		
1874. April 1	4	6	53	44.3	S.	Disappearance seen through light cloud.
" 1	7	7	23	52.5	S.	Star very faint on limb.
" 5	8	8	9	32.0	...	Good observation. Disappearance pretty sudden.
" 5	7	8	17	16.4	...	Very good observation. Disappearance sudden.
" 5	8	8	20	32.4	S.	{ Pretty good obs., but uncertain, perhaps to half a second, owing to star's faintness on limb.
May 12	8	8	23	51.5	Moon totally eclipsed.	{ Disappearance pretty sudden.
" 12	$8\frac{1}{2}$	8	35	45.3		{ Very faint at disappearance.
" 12	8	8	37	5.8		{ Star appeared to advance slightly within limb.
" 12	$8\frac{1}{2}$	8	53	59.3		{ Approximate, star being extremely faint.
" 12	9	9	11	54.3		

Date of Obs.	Mag. of Star.	Observatory Mean Time of Disappear- ance.	Quad- rant.	Remarks.
		h m s		
1873. June 2	7½	9 13 24.6	N.	Uncertain to half a second.
" 2	7½	9 22 48.5	S.	Very good observation.
" 3	7½	6 34 7.3	N.	
" 5	7	10 8 8.4	N.	Disappearance pretty sudden.
" 5	7	10 16 38.4	S.	Ditto ditto.
" 8	5	9 2 4.5	S.	{ Very good observation. The star was distinct, and disappeared apparently by two stages.
July 31	7½	6 30 43.3	S.	Disappearance instantaneous.
Aug. 27	7	7 26 38.6	N.	Disappearance pretty sudden.
" 28	8	6 51 27.0	N.	Star very faint. Obs. uncertain to 1 second.
" 28	7½	7 1 24.1	...	Good obs. Disapp. not quite instantaneous.
" 28	7	7 50 6.4	S.	Obs. very good. Disappearance instantaneous.
" 28	7½	7 50 41.9	S.	Ditto ditto.
" 28	8	8 24 12.9	S.	Ditto ditto.
" 29	7½	6 32 19.7	N.	Good observation. ditto.
" 29	7½	6 44 58.3	...	Good obs. Disappearance not quite instantaneous.
" 29	7½	7 1 31.0	N.	Star faint. Uncertain to a quarter of a second.
" 29	7½	9 27 53.0	...	Good obs. Disappearance sudden.
" 29	7½	9 44 21.9	S.	Ditto. ditto.
" 30	7	7 1 21.7	N.	Obs. very good. Disapp. not quite instantaneous.
" 30	7	8 11 42.9	N.	Ditto. Disappearance instantaneous.
" 30	7	8 42 33.9	N.	Ditto. ditto.
Sept. 2	2	13 27 17.2	N.	{ Disapp. instantaneous, but obs. uncertain to a considerable fraction of a second.
" 30	7	8 48 18.4	N.	
Oct. 27	7	7 38 56.1	...	
" 27	8	7 40 27.7	...	Star faint.
" 27	7	8 34 53.6	N.	
" 27	8	9 1 57.5	N.	
" 28	7½	8 39 57.1	S.	Very good obs. Disappearance instantaneous.

Eclipses of Jupiter's Satellites, 1873.

Date of Obs. 1873.	Satel- lite.	Phase of Eclipse.	Observatory Mean Time of Phase.	Remarks.
			h m s	
April 4	IV.	D	10 17 14.0	Tolerable definition, but disapp. very gradual.
" 6	II.	R	6 32 25.5	Sky beautifully clear. Good observation.
" 21	IV.	R	8 52 41.3	Obs. late, owing to a passing cloud.

Date of Obs.	Satel. lite.	Phase of Eclipse.	Observatory Mean Time of Phase.	Remarks.
			h m s	
1874. Apr. 25	I.	R	8 37 47.3	Cloudless evening. Excellent definition.
May 11	I.	R	6 56 57.6	{ Moon near opposition. Observation through thin filmy clouds.
„ 15	II.	R	8 50 23.4	Good definition. Bright moonlight.
„ 18	I.	R	8 51 52.1	Sky slightly hazy. Pretty good definition.
„ 27	I.	R	5 15 47.7	{ Twilight. Beautifully clear and definition pretty good.
June 3	I.	R	7 11 0.8	{ Good definition, and sky beautifully clear. Moon in first quarter, and not far from planet.
July 12	I.	R	5 44 4.1	{ Sky clear, but planet boiling and somewhat tremulous. Belts pretty distinct.

Windsor, New South Wales, 1874, April 9.

*Observations of the Zodiacal Light.**

By Vincent Fasel, Esq.

I would beg to submit to the Astronomical Society the following report on the Zodiacal Light, which, in the early months of the present year, has been displayed in this locality (Morges, Switzerland) with no ordinary brilliancy, and for that reason I feel the more induced to transmit my own observations, with the hope that they may prove interesting, and perhaps be of some value for comparison with those recorded in other distant places.

The first opportunity I had of examining the luminous phenomenon was on the evening of the 5th of February last, at 8^h p.m., L.M.T., when it exhibited the usual figure of a slightly inclined cone, with a very faint vertex, reaching, on this occasion, to a line drawn from θ *Arietis* to ξ^2 *Ceti*. The light was very distinct and bright, specially on the axis and towards the horizon, but the outline could not easily be made out. A line drawn from θ to γ *Arietis*, passing near to and to the south of ρ *Piscium* and γ *Pegasi*, down to the horizon, would indicate the northern boundary; the southern one, though not well defined, the light fading off gradually near it, passed from a little south of ξ^2 *Ceti*, taking in α *Piscium*, to about ϕ^1 *Ceti*. Sky clear, a fine starry night, and *Mars* shining brightly in a mass of diffused light. February 6th, at 7^h 15^m p.m., another fine clear evening; the Zodiacal Light more conspicuous than on the previous day, with a better defined conical figure. The contour of the vertex, extremely faint, could but just be traced out to π and μ *Arietis*. The light appeared quite as bright as the *Milky Way*, and with a

* The Paper was accompanied by two Drawings.—Ed.

greater intensity nearer the horizon and about *Mars*, which was nearly situated on the axis. At 7^h 20^m just observed a meteor crossing *Aries* from North to South; it was of the second magnitude, and of a red colour.

February 7th, at 7^h 18^m p.m., the Zodiacal Light very distinct, but not so brilliant in the vicinity of the vertex, whilst nearer the horizon it was remarkably luminous, and appeared to have a broader base—quite 30°; *Algenib* within the northern boundary, and 37 *Ceti* just outside the southern. Sky hazy and air calm.

February 9th, at 7^h p.m., the Zodiacal Light unusually brilliant. The smaller stars within it were scarcely visible, and the larger ones, with *Mars* itself, appeared evidently dimmed by its intensity. Yet, notwithstanding that great degree of luminosity, the curvature of the vertex was not easy to trace out. A drawing of the appearance was made on the spot, which will speak for itself, and where it will be seen that the axis lay *close by* and *parallel* to the ecliptic. The light was yellowish white, surpassing in vividness that portion of the *Milky Way* that runs through *Cassiopeia*, which was well situated for comparison. The air very clear, and sky cloudless, except over the distant Alps, a few clouds just crowning the summit of Mont Blanc.

February 10th, at 7^h 14^m p.m., again a vivid appearance of the phenomenon, the conical figure well defined, and *Mars* still situated nearly on the axis. The extent of the light and general features pretty much the same. The air remarkably clear, and the 'bise'* rather sharp. In the early part of the day snow fell.

February 11th, at 7^h 10^m p.m., the Zodiacal Light again conspicuous, but not so bright nor so well defined as on the previous evening. The vertex hardly perceptible, extending only to a line drawn from ρ to σ *Piscium*. While gazing at it a fine meteor of the first magnitude, of the same colour as *Mars*, and with a short train, shot from East to West, within the zodiacal gleam, running with moderate velocity nearly parallel to the axis; passed close by *Mars*, the latter dividing its course into two equal parts. The extent of its path was estimated to be equal to a line drawn from *Algenib* to *Markab*. The effect was very pretty.

Between this and the 5th of next month an interval of cloudy or moonlight nights followed, during which one could now and then catch glimpses of the luminous gleam.

March 5th, at 7^h 30^m p.m., a very favourable view of the Zodiacal Light was obtained, the atmosphere being remarkably transparent and the sky perfectly cloudless. The northern boundary passed through γ *Arietis*, south of ρ *Piscium*, and just close to γ *Pegasi*; the southern, from ξ 2 *Ceti*, passed between α and ξ *Piscium*, directly to the horizon. The vertex reached δ *Arietis*. The light appeared considerably brighter than the *Milky Way*. *Mars* was a little to the south of the axis, between γ and ϵ *Piscium*. At 7^h 40^m a small meteor crossed *Perseus* from North to South.

* North wind.

March 6th, at 7^h 35^m p.m., sky very hazy and clouds round the western horizon. I could but just catch a glimpse of the upper part of the cone.

March 7th, at 7^h 50^m p.m., the southern boundary marked by α *Piscium*, and ξ^2 *Arietis*. The vertex very faint, and appeared to reach ζ *Arietis*.

March 8th, at 8^h 30^m p.m., the light decidedly more brilliant than on the two preceding nights. The northern boundary was boldly marked between γ and β *Arietis*, and the vertex extended nearly to the *Pleiades*.

March 10th, at 8^h p.m., as the twilight disappeared the Zodiacal Light became discernible, presenting the same features as before with regard to the extent and luminosity. The vertex appeared to involve τ^1 and τ^2 *Arietis*. *Mars* about the middle of the cone, and somewhat to the left of the axis.

March 14th, at 7^h 45^m p.m., an exceptionally fine evening, the sky perfectly cloudless, and the air calm; such were the conditions under which I witnessed a fine display of the Zodiacal Light. It was brighter and more conspicuous than I have ever seen it on any former occasion. Its colour was of a yellowish white. A line drawn from ρ to σ *Piscium* would show the extent of the brighter portion of the cone up to ν , μ , π *Arietis*. Another drawing of the appearance was made, as on the 9th of last month. I traced the boundaries on maps 2, 3, and 4 of Mr. Proctor's *New Star Atlas*, and was pleased to see that it could be done with sufficient accuracy to give a correct idea of the display. These two tracings I beg to enclose along with the above report, with the hope that they will prove acceptable, and will also excuse me for prolixity.

My place of observation was the same as that described in the *Monthly Notices*, Vol. xxxiii. p. 100.

Morges, Switzerland,
1874, *June.*

P.S.—Position of *Morges* approximately.

46	30	0	N. Latitude.	
6	31	0	E.	} of Greenwich in Longitude (mean time).
h	m	s		
or 0	26	4	fast	

Discovery of Minor Planet (138). By M. Perrotin.

This planet was discovered at the Observatory of Toulouse on May 19. The observed places on May 19 and 20 are as follow :

1874.	Mean Time at Toulouse.	R.A.	N.P.D.
	h m	h m s	° ' "
May 19	10 0	16 28 30	112 48
20	12 0	16 27 28	112 47

Definitive Elements of the Comets I. 1801; I. 1824; III. 1840;
II. 1869. By Dr. Doberck, Astronomer at Colonel Cooper's
Observatory, Markree Castle.

I. 1801. T = 1801, August 8^h 56^m 30^s. M. Par. T.

$$\left. \begin{array}{l} \pi \quad 182 \ 41 \ 52 \\ \Omega \quad 42 \ 28 \ 54 \\ i \quad 20 \ 45 \ 0 \end{array} \right\} \text{M. Eq. 1801.0.}$$

$$\log q \quad 9.40894$$

Retrograde.

I. 1824. T = 1824, July 11^h 51^m 42^s 30. M. Par. T.

$$\left. \begin{array}{l} \pi \quad 260 \ 18 \ 41 \\ \Omega \quad 234 \ 20 \ 41.0 \\ i \quad 54 \ 36 \ 44.8 \end{array} \right\} \text{M. Eq. 1824.0.}$$

$$\log q \quad 9.771850$$

Retrograde.

III. 1840. T = 1840, April 2^h 42^m 10^s 53. M. Par. T.

$$\left. \begin{array}{l} \pi \quad 324 \ 3 \ 52 \\ \Omega \quad 186 \ 2 \ 73 \\ i \quad 79 \ 52 \ 29.8 \end{array} \right\} \text{M. Eq. 1840.0.}$$

$$\log q \quad 9.8743460$$

Direct.

II. 1869. T = 1869, October 9^h 56^m 13^s. M. Par. T.

$$\left. \begin{array}{l} \pi \quad 139 \ 42 \ 33.25 \\ \Omega \quad 311 \ 30 \ 10.4 \\ i \quad 111 \ 40 \ 21.5 \end{array} \right\} \text{M. Eq. 1869.0.}$$

$$\log q \quad 0.09016325$$

*Périodes de Révolution de 4 étoiles doubles, d'après les dernières
Observations.* Par M. Flammarion.

	Ans.	Demi gr. axe app.	Passage Périhélie app.
ξ Ursæ Majoris	60.60	2. 45	1873.40 à 358°
ζ Herculis	34.57	1. 19	1864.35 à 298
η Coronæ Borealis	40.17	0.865	1853.95 à 287
γ Virginis	175.	3.389	1836.45 à 320

M. Flammarion remarks that, owing to the large number of observations used in the calculation, he believes the above periods to be absolutely exact.